(3.)

A
DISSERTATION
ON THE
MOTION of the BLOOD,
AND ON THE
EFFECTS of BLEEDING.

VERIFIED BY
EXPERIMENTS made on Living Animals.

To which are added,
OBSERVATIONS on the HEART,
proving that IRRITABILITY is the
primary Cauſe of its Motion.

By the Celebrated
Albrecht von
DR. ALB. HALLER, d. 1777.
Preſident of the Royal Society of Sciences at
GOTTINGEN, Member of the Royal Academy of
Sciences at PARIS, Fellow of the Royal Societies of
LONDON, BERLIN, STOCKHOLM, &c.

Tranſlated by a PHYSICIAN.

LONDON:
Printed for J. WHISTON and B. WHITE, in Fleet-ſtreet.
MDCCLVII.

THE

PREFACE.

THE learned author of this diſſertation preſented it to the Royal Society at GOTTINGEN, the eighth day of October, 1754; and it is publiſhed in the fourth volume of the Tranſactions of that learned aſſembly. He calls it an Analytical Expoſition of Reſults; and theſe he hath verified by two hundred and thirty-five experiments made on living animals. Hereunto is added a ſhort diſſertation, containing the author's experiments made on the heart, proving that irritability is the primary cauſe of its motion. A tranſlation of the experiments themſelves, as preſented to the ſaid ſociety, the twenty-ſixth of March, 1756, and which are the foundation of this diſſertation, will be very ſoon publiſhed.

ON THE

CIRCULATION of the BLOOD.

YOU will excuſe, good ſirs, my laſt year's ſilence, if you conſider my ſudden and unexpected departure, the length of the voyage, the difficulty of conveying my books, the fatigue of a new eſtabliſhment, and laſtly an unhappy fall that deprived me for many months of the uſe of an arm, I did not, notwithſtanding all this, forget you nor the duties I owe you: and indeed how could I forget you, with whom I have paſſed my time ſo agreeably? you who, by your ſtrict union and continued labours during the laſt years of my reſidence at Gottingen, gave me much pleaſure and ſatisfaction? and laſtly you, gentlemen, whoſe friendſhip is a ſenſible part of my happineſs, the abſence whereof, I have always regarded as an irreparable loſs? Could I forget my duty when called upon, and by an honourable office our prudent protector had been pleaſed to preſerve for me, and through my deſire of enquiring after truth? could I forget a ſociety, to whoſe buſineſs I ought juſtly to ſubmit, ſince I enjoy its honours? For theſe reaſons, gentlemen, I have been conſidering what ſubject I could make choice of, that would equally come under your cognizance as well as mine, having changed my courſe of ſtudy as to the anatomy of the human body. I think now the only thing remaining for me, is the diſſecting of living animals. I have opened a great number at Gottingen, an account whereof I have in my papers, and have added thereto diſſections that I made at Berne, to divert me, by the ſtudy of ſciences, from other buſineſs of civil life that is leſs engaging. To theſe diſſections, I add microſcopical obſervations of cold animals, one hundred whereof I have ſacrificed to my enquiries, partly with Dr. Remus and others, ſince my return into the country. I ſhall take another opportunity of informing you what experiments have taught me concerning reſpiration and generation; and here I ſhall only take notice of the nature of the arteries and veins, the globules of blood, their motion in the veſſels, the cauſes of this motion, the changes that ligatures and bleeding can produce therein, and occaſionally the changes the blood can undergo. Theſe ſeveral articles I propoſe as the ſubject-matter of as many chapters.

CHAP. I.

Concerning the Structure of the Arteries and Veins.

I Have not very attentively examined the great veſſels of animals that have cold blood, yet having obſerved the Aorta to go out double from a thick ſolid trunk and cellular internally, I traced it even to the Meſentery; and I obſerved the pulmonary arteries, which are not large, and are only of a diameter proportional to that of this viſcus, to ariſe from the deſcending branch. I apprehended this viſcus ſomewhat like a bladder for ſwimming, each lobe incloſing a conſiderable cavity, whoſe circumference is ſurrounded by a ſeries of polygon veſicles, interſperſed with little arteries, agreeable to Malpighy's deſcription of the ſame; they differ from the veſicles of the lungs of men, by their magnitude and by their angular figure, and are themſelves ſurrounded by other veſicles much ſmaller, which one can hardly diſtinguiſh, and which are not always ſeen, and which render the lungs of frogs ſomewhat like thoſe of man.

In the Meſentery of theſe animals, the arterial trunks extend themſelves direct enough towards the Inteſtines, where they divide and ſpread themſelves in a ſerpentine manner upon their ſurface, or uniting themſelves one with another, they make rings reſembling thoſe which the colic and ileocolic artery form on the human Colon. We can ſee diſtinctly, in theſe ramifications, that the total of the lights and cavities of two branches from one trunk, is greater than the light of the trunk itſelf; and as far as I could obſerve with a microſcope, the arteries in the Meſentery are conical: although during the time the animal ſuffers under the experiment, their diameter may be often changed and diſfigured, and undergo different ſtrangulations, as the blood is propelled more towards one place than another.

The veins almoſt like the arteries, exceed them in number and diameter, which is almoſt double that of the arteries; but the chief difference of theſe two kinds of veſſels, is in the network of the veins. Their trunks ſubdivide themſelves ſucceſſively into very ſmall ramifications of the diameter of a ſingle globule, and mixing themſelves with other veins of the Meſentery, form a venal network, without any one branch of an artery *(a)*. Theſe veins are apparently cylindrical, with frequent flexions, their angles of re-union are pretty large, and the areas that ſeparate them are polygons, ſomewhat reſembling a ſquare. Generally ſpeaking, I have not obſerved in my experiments, any arteries of a globule diameter, becauſe perhaps the thickneſs of the membranes hinders one from perceiving the faint redneſs of a ſingle globule; the apparent white colour of theſe ſmall veſſels confounds them, with the membranes on which they creep; the great arteries themſelves are pale when the veins appear very red, without any mixture of blue. As to the tails of fiſh, I have ſometimes obſerved between two parallel arteries, arterial plexuſes, whoſe veſſels appeared of the ſize of a globule of blood.

On examining carefully the membranes of the arteries, they will be found thick, white, and compact. Their thickneſs hath not always the ſame appearance with the whole diameter of the artery; and this thickneſs often makes one half of the diameter, and the cavity of the canal the other half: ſometimes it makes the greater part of it. It happens that the blood is propelled with more force in the arteries, without any

---

(a) D. Heide de Sang. Miſſione, p. 8. & Lewenhoeck Cont. Arcan. Nat. 3. p. 62. ſpeak of this network.

dilation of the whole diameter, becauſe the blood poſſeſſeth the greater part of their light; and the parieties of the arteries are reduced to a third or a fourth part of their firſt thickneſs. By this it appears, that the denſity of the arteries increaſeth in proportion to the force with which the blood is propelled therein. The greater ſpace the blood poſſeſſeth in a circle, the leſs remains for the parieties, and conſequently the more the fibres of theſe parieties muſt be compreſſed. This, if I miſtake not, helps us to account for the hardneſs of the pulſe in ſome fevers, yet not that I would exclude the blood's increaſed denſity.

All like the arterial tube may be full beyond meaſure; but it may happen alſo that a ſufficient fulneſs may be wanting. Nothing happens more frequent than to find in frogs empty veins, and alſo arteries either quite or half empty, or even which contain not more than one, two, or three columns of blood globules; and it is not conſonant to experiments, to believe that the veſſels contracted in proportion to the quantity of blood they contain are always full, and that they only become narrower when they contain but little blood: for although faſting diminiſhes the quantity of blood, yet the diameter of the blood-veſſels is not leſſened; and often bleeding or other cauſes, re-eſtabliſhes the courſe of the blood that was ſuſpended in ſome arteries, thereby reſtoring to them their former plenitude.

But there is another change in the diameter of the artery that frequently happens, and which I have often produced by pricking the artery, and which at other times, I have obſerved to happen from ſome cauſe unknown to me; I mean the true Aneuriſme which is found ſo often in the arteries of frogs examined with a microſcope, and which is a tumour almoſt oval, which the ſuperior part of an artery ſupplies with blood, which blood this tumor conveys to the inferior part of the ſame trunk. I diſagree with ſome authors who are of opinion, that the true Aneuriſme ought to be excluded from the number of chirurgical maladies *(b)*. I have ſeen it happen, as I have before related, from ſome cauſe unknown to me *(c)*; and I have ſince learned how to produce it at pleaſure. For this purpoſe, I ſeparate the two laminæ of the Meſentery from the two coats of an artery, after which I ſhake it, ſo as entirely to diſengage it from its connecting cellular ſubſtance; after this I ſoon find an Aneuriſme produced. It is

---

(b) See on this diſpute Dr. Friend's Hiſtory of Phyſic, p. 184. the quarto edition of Paris 1735.

(c) Theſe Aneuriſms Lewenhoeck ſaw, when he declared the veſſels grew thicker, where the blood began to coagulate.

alſo equally formed after an inciſion, and eſpecially after the pricking of the artery: ſometimes alſo, though not ſo often, the like tumours are obſerved in the veins; but chiefly in theſe we find, as I have a little before related, inequalities in their diameters, which is the reaſon that that part which is neareſt the heart, is not always the largeſt.

I have not perceived ſmall veſſels upon the membranes of the arteries; but in frogs there is often found on the membranes that compoſe the veſſels of every order, particular black ſpots a little like arrows, formed in network, the cauſe whereof ſeems very uncertain. The veins have ſo delicately thin a membrane, that one may ſee very diſtinctly the blood globules; they ſeem to form a kind of chain like a row of beads, ſomewhat like Cowper *(d)* and Cheſelden's *(e)* deſcriptions of the ſmall veſſels; but the courſe of theſe veſſels they take no notice of. This made Lewenhoeck ſay, that the blood-veſſels have no membranes, and that the red globules make their own way into the places where they find the leaſt reſiſtance *(f)*. I have never yet obſerved any valve in the veins of frogs.

I have never been able, even by the help of a microſcope, to diſcover any muſcular

---

(d) Append. ad Bidl. t. 3. fol. 3.

(e) Anatomy of Human Body, edit. 6. t. 3.

(f) Experiments and Contemp. p. 184.

fibre in the veſſels of the Meſentery; altho' the veins near the heart are certainly provided with them; for we can very diſtinctly obſerve in both Venæ cavæ motions of conſtriction correſponding to thoſe of the auricle.

Thoſe red ſpots occaſioned by extravaſated blood, taken notice of upon all kind of veſſels after wounds, and eſpecially upon the human arteries after an inflammation; theſe ſpots, I ſay, demonſtratively prove that all theſe veſſels are accompanied with ſome cellular ſubſtance, which, by reaſon of its tranſparency when empty, eſcapes our view. It is not uncommon to ſee capillary veins, lying connected upon the arteries in the major part of their courſe, and without interrupting in the leaſt the motion of their fluids; and this teacheth us how little we ought to regard the reaſon that is given, why the left ſpermatic vein is inſerted into the Emulgent rather than into the Cava. It is, ſay they, a precaution of nature, leaſt the pulſations of the Aorta ſhould obſtruct the cavity of ſo ſmall a vein, or diſorder the courſe of its blood. The weakneſs of this theory may be again proved by the human Aorta, which, in all the cavity of the breaſt, is ſurrounded with ſmall veins ariſing from the ſuperior Intercoſtal; and yet notwithſtanding their being ſo near ſo large a veſſel, receive thereby no detriment to their functions. Hereupon may be conſulted the deſcription and figures, that I have publiſhed of the bronchial artery.

I have not traced in frogs the ſmall veſſels which form the continuations between the arteries and the veins. The Meſenteries of theſe animals are not proper for this obſervation, becauſe this union ſeems to be made in the membranes of the Inteſtines, which are too opaque for obſervation: but this is very diſtinctly ſeen in fiſh; and as I have in a manner obſerved nothing but what is already known, I ſhall be ſhort on this article. In the tail of ſome ſmall fiſh we ſee four little bones, each whereof is accompanied with an artery and a vein. The conveyance of blood from the one to the other, is made in two different manners; often the artery bends itſelf even to the extremity of the tail, and bending like a hook, turns parallel to itſelf. By this ſimple method, a number of arteries, even of the diameter of many globules (*g*), become veins: therefore it is no wonder if air, ſewet, and in general the liquors injected by anatomiſts, find ſo eaſy a paſſage through the lungs, reins, Meſentery, heart, and even the human brain, and paſs from the arteries

---

(*g*) Lewenhoeck's Experim. and Contem. t. 11. p. 177, limits the number to three, but I have ſeen the contrary.

of theſe viſcera into their correſponding veins. This is the firſt method of union, deſcribed by Lewenhoeck in different parts of his works (*h*). There is yet another method more complex; it proceeds from an artery under different angles; many branches of one, two, or three globules diameter terminating themſelves in a parallel vein, ſupply it with arterial blood. This laſt manner of communication, is as often obſerved in fiſh as the firſt. This Lewenhoeck hath ſpoken of (*i*).

What remains for me now to enquire after, is what the veſſels are which Dr. Boerhaave (*k*) appoints for the conveying of the liquids thinner than blood. A perſon not accuſtomed to obſervation, eaſily perſuades himſelf he ſees them, when he obſerves for that purpoſe, in the Meſentery of frogs, little veſſels which ſuffer only a yellow globule to paſs them. Deceived by this colour, I thought myſelf happy ſome years ago, in having it in my power to make this obſervation; and Mr. Baker (*l*) mentions the ſame error in a friend of his: but an obſerver leſs ſubject to believe what he deſires, does not long enjoy this pleaſure, and ſoon

---

(*h*) Ibid. p. 106. f. 6. A. f. 10.

(*i*) T. Sive Exper. & Contem. p. 178. f. 1. p. 183. f. 14. p. 185. f. 15. p. 186. f. 16. 17. and in other places.

(*k*) De Uſu ratiocinii mecanici, p. 14.

(*l*) Microſcope made eaſy, p. 136.

confeſſeth that all he ſaw, were only very ſmall veins ariſing from red veins, which uniting become again red veins, containing only red globules. If to the eye they appear yellow, it is only becauſe the colour of a ſingle row of globules being extremely weak, vaniſhes and diſchargeth itſelf through the membranes, and is not apparent to us, until the globules become more numerous. The eminent M. Senac and an anonymous author, have mentioned this before (*m*).

Notwithſtanding this, I will not advance that there are no veſſels ſmaller than the red ones; all that I eſtabliſh is, that I have obſerved nothing that contradicts a poſſibility of their being arteries, and canals, containing a liquor different from blood, and that there may be other veſſels appointed for the circulation of a thinner fluid; for we obſerve them, in the muſcles of eels of a ſilver colour, containing a humour different from the blood. There are many other reaſons to confirm this opinion.

(*m*) Lettre ſur le Nouveau Siſteme de la Voix. p. 55.

CHAP. II.

Of the Humours.

IT remains for me now to ſpeak of the fluids contained in the arteries and veins. Of theſe I have hitherto obſerved two kinds, the red globules and an inviſible liquid in frogs and fiſh, which is called in large animals by the name of limph or ſeroſity. I will begin with the red globules, the diſcovery whereof ſeems to me to be owing to Malpighy, although he (*a*) conſidered them in a different light. What I call globules are little lentils red or yellow; frogs and fiſh have them of both theſe colours. They are yellow in animals becoming feeble, and are of a deeper red on the animal's recovering ſtrength; and this redneſs doth not ſolely depend, as ſome authors imagine (*b*),

---

(*a*) In his treatiſe De Omento & Adipoſis Ductibus, which he publiſhed in 1665, he deſcribes, p. 42. of the London edition, a blood veſſel of the Omentum, in which he ſaw globules of fat circumſcribed, red and pretty like grains of red coral. This deſcription ſeems to intimate the red globules; and it was only the 15th of Auguſt 1673, that Lewenhoeck took notice of the red globules of the blood. Tranſ. Philos. Nº 102.

(*b*) One may ſuppoſe it is the opinion of Queſnai by what he ſaith. Œconom. Anim. Tom. III. p. 36. See what the celebrated Senac ſaith thereupon, in his Traitè du Cœur, tom. II. p. 662.

on the re-union of a great number of globules, I having often obſerved, in the ſmalleſt veſſel, ſolitary ſingle globules very red, moving in a row, and at the ſame time a collection of a great number in the large veſſels, appearing yellow and even pale (*c*).

The ſize of theſe globules is very ſmall; for, on obſerving with the ſame ſolar microſcope one globule, and a little feather of the wing of a butterfly, the globule appeared to me at leaſt one thouſand times ſmaller; and I ſhould be very much inclined to admit the Engliſh calculations, which make 1940 (*d*) or even 3240 diameters of a globule to make an inch: and I think this too low a calculation. On examining them with a glaſs, magnifying the diameter of the object 250 times, their diameter hath not appeared to me larger than the twentieth part of an inch, which gives, with reſpect to this diameter, to an inch 1,5000.

There hath been lately ſome doubt with reſpect to their figure, whether they were ſpherical, or whether their different diameters were unequal. Lewenhoeck (*f*) and G. W.

(*c*) Theſe are the veſſels, in which M. de Sauvage ſaith are only found yellow globules. Pulſus Theor. p. 24.

(*d*) Jurin. Philoſ. Tranſac. Nº 377, art. 7.

(*e*) Hale's Hæmaſtat. p. 56. in his remarks.

(*f*) Experim. & Contem. T. II. p. 73. & ſeq.

W. Muys (*g*) have obſerved, that thoſe of fiſh and of other animals that have cold blood, were flat and at the ſame time oval, ſo that each globule had three different diameters; the greateſt is that of its length; the magnitude makes the mean diameter, and the thickneſs makes the ſmalleſt. M. Miles (*h*) and M. Senac, (*i*) have in general given them the figure and form of a lentile; this laſt eſpecially ſaith, they are eliptic and flat in frogs: he imagines even to have obſerved an edge almoſt tranſparent in their circumference.

I have often examined them, and particularly the half circumferences of the globules, which ſeem to form a ſmall eminence upon the membranes of the veins; they appeared to me thick in frogs, and obſerving the variations that were produced by different rays of light, the diameter of the thickneſs appeared to me to be equal to that of the length, or at leaſt very near the matter; the diameter of the magnitude did not ſeem to be leſs. It is very difficult to give any poſitive opinion of any object ſo ſmall and ſo moveable; what I can ſay with certainty is, that always when I ſaw globules extravaſated between the membranes of the Myſentery,

---

(*g*) Fabric. Muſculor, p. 300. & ſeq.
(*h*) Tranſac. Philoſ. N° 460, p. 726.
(*i*) L. c. p. 656.

I never could obſerve one diameter greater than another; but I am in ſome doubt whether my eye hath deceived me. Thoſe who have made uſe of the ſolar microſcope, have alſo obſerved the ſpheric Moleculæ, although this inſtrument renders the Peripheries leſs diſtinct than the ordinary microſcopes. Mr. Baker, on obſerving theſe globules with this microſcope, compares them to grains of pepper (*k*)

But you will aſk, if they change their figure? Many authors of credit long ago have reported (*l*), that the compreſſion which the globules undergo in paſſing into the extremities of the ſmaller veſſels, changeth their figure; that the diameters become unequal, the longeſt being that which is parallel to the axis, and the ſhorteſt that which is parallel to the diameter of the veſſels. I do not deny my having ſeen ſomething of this nature, eſpecially when the globules paſs through curves and flexions of the ſmall veſſels; but I do not care to aſſert this as an abſolute truth.

(*k*) Philoſ. Tranſac. N° 458, p. 517.

(*l*) Lewenhoeck Exper. Contem. p. 61, and in other places; James Keill of muſcular motion, p. 163; H. Miles' Philoſoph. Tranſac. N° 460; M. Senac Traite du Cœur, l. 2. p. 657. The celebrated Van Swieten, ſaith he hath obſerved it in the pulmonary veſſels of water lizards. Comment. in Aphoris. Boerhaave, Tom. I. p. 145, F. W. Horch Miſcel. Berolinens. Tom. VI. p. 116, and many other authors.

I find the late Mr. Platner *(m)* too poſitive; and all who like him have denied theſe globules, or have taken them for adipoſe or fatty maſſes, ſince they are certainly diſtinct circumſcribed Moleculæ, of a different nature from the other hunours, always like themſelves, and intirely independent of any accidental formation.

Whatever ſome authors *(n)* have imagined of them, it will be found very improbable that they contain air, if one does but conſider their ſolidity, which is ſenſible to the eye, and what we know in other reſpects, that the red part of the blood is the heavieſt *(o)*, and ſinks to the bottom of the ſeroſity *(p)*: and indeed one could not think otherwiſe of thoſe globules that contain ſome particles of iron, whoſe ſpecific gravity is to that of the ſeroſity, as 7 to 1. Another proof that they contain no air is, that when a candle is brought near the Meſentery of a frog, ſo as to

---

(m) Diſſert. de Noxis cohibit. Suppurat. N° 6. Hartſoezer Extraits. p. 5.

(n) Bohn. Circ. p. 179. George Cheyne's Philoſophical principles of Religion, p. 304. G. Erchard Hamberger Phyſiolog. Medic. p. 16, 17. T. Knight's vindication, p. 4. and ſome others.

(o) Jurin. Diſſert. 8. p. 99, gives the proportion of the weight of the red globules to that of the ſeroſity, as 1054, 1030, and the celebrated M. Thomas Schwenke as 1240, 1142, Hæmatolog. p. 123.

(p) Morgan's Philoſophic. Princip. p. 114; ſaith a drop of blood falling into water diſengageth itſelf of its fat, and ſinks to the bottom.

heat the blood, even almoſt ſo near as to burn it. The globules, do not at all dilate themſelves, which they would conſequenly do if they contained air: neither do they at all dilate themſelves in the receiver of an air-pump *(q)*, when the air is drawn off. From hence we learn what we ought to think of any hypotheſis, founded on a ſuppoſition, that the globules are bullæ full of air.

I have obſerved many changes theſe globules undergo. The firſt, which happens very eaſily, is their coagulation; this is formed from clots of blood, by the re-union of eight or a greater number of globules, which very often ſhuts up the paſſage of the blood in the veſſels, eſpecially in the places where there happens to be Aneuriſms or Varices; yet the ſtrength of the heart prevailing, propels theſe grumous parts, and thereby makes the paſſages free: this happens in large Aneuriſms. I have ſeen one in the neck of a very great magnitude, filling all the carotid artery; I found this Aneuriſme filled with a thick and grumous blood. Some celebrated writers have taken notice of this before me (*r*); and Lewenhoeck (*s*) hath obſerved this in frogs.

(q) Jurin. Diſſert. p. 100.

(r) With reſpect to the Varices, M. de Sundrie de Natura Sanguinis, p. 218. With reſpect to the Aneuriſms, the Memoirs of the royal Academy of Sciences, anno 1707, p. 21.

(s) Experim. & Contemp. p. 179.

The ſecond change the red globules undergo, is when they loſe their motion in the veins; they then ſeem to be changed into oil, and afterwards into a kind of ointment, wherein one may obſerve a few ſtriæ or lines, like what is obſerved in diſtilling oils: but in the great number of experiments which I have made on frogs, I never found that they formed themſelves into polygon plexuſes like network. This change, however, doth not deſtroy the nature of the globules; for bleeding or heat deſtroy this unctuoſity, and eſtabliſheth a diſtinction between each globule: but when the globules are ſtrayed out of their veſſels, either arterial or venal, not only in animals that have warm blood, but alſo in frogs, they become grumous.

Another conſiderable change of the blood, is the formation of the lumps and clots that ſhut up wounded veſſels. I intend to ſpeak of this, after I have ſaid ſomething concerning the tranſparent part of the blood, of which one can have no idea by obſerving the veſſels in a ſtate of plenitude. For in the arteries and veins of a frog in health and well fed, the red globules ſeem ſo entirely to fill them, that one would not imagine they could contain any thing beſides. M. V. Menghini (*t*) believed there was proportionally fewer red globules in frogs than in men; but I cannot

---

(*t*) Commentar. Acad. Bonon. F. 2. p. 2. p. 238.

conceive how one could add more of theſe red globules to the veſſels, ſince they already ſo fill the canals as to prevent a diſcovery of any one tranſparent point: and M. Pujati (*u*) is much in the right in ſaying, that the tranſparent ſeroſity, in which the globules ſwim, is not to be perceived.

But notwithſtanding this impoſſibility of ſeeing in ſome caſes this fluid, it is eaſy to demonſtrate as to the ſmall veſſels, that they contain a very fine and thin fluid. It is ſufficient for that purpoſe, to obſerve the conſiderable intervals that appear in a row of theſe globules, which are often found diſtant from one another, and yet are in continual motion. This ſeems an evident proof, that there is between theſe ſeparated globules, a fluid which ſerves to impart from the one to the other, even to thoſe of the extremities, the moſt diſtant from the heart, a motion which they impreſs to thoſe that are in immediate contact with them: moreover we ſee very frequently in large arteries, ſpaces that appear empty, and in which the red globules poſſeſs but a very ſmall part of the cavity of the veſſel. One may ſuppoſe, that this apparent empty ſpace muſt neceſſarily be filled with ſome tranſparent liquor. How can it happen that membraneous veſſels, ſuch as the arteries and veins, ſhould remain in a ſtate of dilation,

---

(*u*) De Morbo. Naronienſi, p. 104.

if they were not full? I have often obſerved red globules drawn towards the cavity of an Aneuriſme, and which were repulſed from the ſame, before they had touched any red blood; a demonſtrative proof that they had met ſome other fluid, which was to me inviſible, and which had reſiſted them. But the exiſtence of this tranſparent fluid, ſhews itſelf more evidently yet; when an artery or vein is opened by bleeding, (for after the firſt force of the blood is over, and it bleeds but ſlowly, one may obſerve a kind of cloud to ſurround the wound or orifice, compoſed of a liquor that by degrees becomes white, and which changeth itſelf into a tumour, which one would think was formed by the very membranes of the veſſel, and in the middle of which one may diſcover a red point, which is exactly ſituate on the orifice of the veſſel) one may obſerve preſently afterwards that the wound cloſes itſelf again under this tumour, and the blood retakes its natural courſe in the veſſel. It cannot be denied, but that this cloud may be formed by a white fluid, ſince it hath been obſerved, that the globules extravaſated between the membranes of the Meſentery or of the arteries, do not undergo the like change, but firſt form little diſtinct grumes, and afterwards the unctuoſity and oily matter before taken notice of: herein is no cauſe of doubt, either with reſpect to the red grume which cloſeth large wounds (*w*), nor with reſpect to this white concretion laſt mentioned (*x*), which gives riſe to polypuſes, and to thoſe white laminæ found ſo often in Aneuriſms, and which I have obſerved to inveſt the internal membrane of the carotid and jugular (*y*).

But I have not been able to diſtinguiſh the parts which conſtitute this white fluid, even with microſcopes that magnify much more than thoſe of Lewenhoeck; and I can hardly believe, that it is poſſible to diſcover any yellow globules ſmaller than the red ones: neither do I think it is poſſible to be certain, that they are parts of the red globules, and that by their re-union, they can again become globules of this colour. This hath created ſome arguments very plauſible (*z*).

(*w*) Andrew Paſta De motu Sanguinis poſt ortum epiſt. 2. Nº 75. Petit Memoirs of acad. royal of ſcien. 1732, p. 394, and 1735, p. 442. M. Morand. ibid. 1736, M. Morand. Med. Eſſ. of the Soc. of Edin. t. 2. p. 273. Mem. of Chirurg. t. 2. p. 537, 542, 544, 117.

(*x*) M. Remus, in the diſſert. before cited. M. Senac t. 2. p. 92. 460.

(*y*) I have given this obſervation in the introduction of the diſpute of M. G. Zuin, and in the Pathological works. There is an account in the Memoirs of the Acad. Royal of Sciences 1732, p. 393, of a falſe Aneuriſme, in which was found a maſs formed of lamellæ, produced from thickened lymph.

(*z*) Lewenhoeck ſpeaks often of globules ſmaller than the red ones. Experim. & Contemp. p. 2, 3, 12, 15, 50; after him Boerhave and almoſt all the phyſiologiſts, have built on this foundation.

But it is impoſſible to ground this opinion on obſervation, ſince thereby there is not any one globule obſerved ſmaller than thoſe of the red claſs; neither does obſervation teach us any thing concerning their reſolution into ſmaller globules. The celebrated M. Senac (*a*) is of my opinion, which I think an honour.

I have never yet obſerved any fat in the blood of frogs; and I ſuſpect very much that what Malpighy thought was fat, was only bullæ of air, like thoſe I have often obſerved circulating in the blood-veſſels with great rapidity, and what Rodi (*b* and Caldeſi (*c*) had obſerved in the veins ef frogs.

I do not conclude from this experiment, that there is air in the veſſels of an animal in health; for in fifty experiments, there will be found only one that ſhall ſhew theſe bullæ of air, and only then when ſome wound hath

---

(*a*) Traitè du Cœur, t. 2. p. 91, 660, he believes the cauſe of this error is, becauſe when one red globule is obſerved alone, it appears ſometimes yellow. The coagulum, which is formed by globules, when at reſt, may have given room for ſome idea of their compoſition, by the re-union of globules of an inferior order; and the opinion that ſuppoſeth the yellow globules, formed from the reſolution of the red ones, ariſeth from the diſſolution of this coagulum, produced either by heat or putrefaction.

(*b*) Epiſtol. ad Stenon. This letter may be found in the ſupplement to Giorn de Litterati, tom. III. p. 86.

(*c*) Obſerv. anat. intorno alle Tartarughe, p. 67.

happened to ſome large veſſel, by which the air introduced itſelf. They are one thouſand times at leaſt larger than the blood-globules; and it is impoſſible therefore, that they ſhould paſs into the ſmall veſſels of one globule diameter. I have not diſcovered filaments in the blood; and it would be impoſſible, that long flexile bodies ſhould, through ſo narrow a baſe, receive a motion ſufficient to overcome the reſiſtances of the ſmall veſſels (*d*).

What I have further to add is, that I have never been able to ſee diſtinctly the globules of animals that have warm blood. If after the manner of Lewenhoeck and Antony Heide, you put your blood into a capillary tube, the parieties of the tube, in adapting the microſcope, creates ſo much obſcurity, that it is impoſſible to diſtinguiſh any thing (*e*). If you extend a mouſe like a frog under Lieburkuhn's microſcope, the opacity of the coats of the Meſentery intirely conceals the veſſels; and the laying bare the veſſels produceth a coagulum of the blood by the cold air: one can only obſerve branches like coral.

No difference is found in the arterial and venal blood of frogs both equally condenſeth itſelf into red grumes; but in a dog and in other animals that have warm blood, I have

---

(*d*) See M. Senac's reaſons, tom. II. p. 103, 104.

(*e*) You find the ſame experiment in the diſſert. of M. Remus, p. 39.

experienced one hundred times that there was no difference, with reſpect to the colour or diſpoſition, to coagulate between the blood of the artery and the pulmonary vein: but in ſome ſingular experiments made on a dog and a great rat, I have obſerved the artery and crural vein to give blood of a different colour; the firſt had very red blood and the vein very black. This difference of colour is not to be attributed to the action of the lungs. I have ſeen a much more remarkable difference in the vein of a frog; it was filled with two ſtreams of blood of dffierent colours; the column which came from the heart was purple, and that which returned from the ſide of the Inteſtines was of a pale yellow colour: and in general, whatever may be the action of the heart and lungs on the blood, the rapidity with which it circulates in the veſſels, and the little time it is in the lungs, and the extreme ſmall time there muſt be for the blood which was a little before venal to become arterial, prove that there muſt be but very little difference between the arterial blood from the pulmonary vein, and the venal blood from the Cava (*f*); and as both this blood contains the ſame globules, their difference can only conſiſt in the proportion

---

(*f*) It is a long while ſince a man born for obſervations, the immortal Harvey, thought no credit was to be given to this difference.

of theſe globules with the limpid fluid, or in the nature of this fluid itſelf: but the white fluids eſcape our ſight; and the experiments that have been made to decide the proportion of globules, to the ſerum in both this blood, do not agree among themſelves (*g*). It is ſeldom that milk and chyle appear in the blood; I have nevertheleſs diſtinctly obſerved a very white chyle running into the axillary vein, and I have attentively obſerved the right auricle, at each contraction, to propel a white liquor into the heart.

CHAP. III.

On the Motion of the Arterial Blood.

THE firſt motive that determined me thoroughly to examine this matter, was the opinions warranted almoſt by all the phyſiologiſts contrary to my experiments. The diſcoveries which I have made by the opening of a great number of living

---

(*g*) Many authors believe the arterial blood more denſe, and among others M. de Sauvage, de Inflamat. p. 244. Others find more water in the arteries and their blood therefore more fluid. M. Ramnarſchmid my pupil, hath found it lighter than very modern experiments. This variety in obſervations ſerve to prove, that if there is any difference in this reſpect, it is very inconſiderable.

living and dead animals, are ſo different from what is generally ſhewed at any univerſity, that I do not expect to ſee them eſtabliſhed, until other anatomiſts have repeated the ſame; and indeed that muſt be done very often, in order to be acquainted with nature, and to learn to diſtinguiſh what is uſual and conſtant, from what is extraordinary and accidental. Always when I obſerved any phænomenon which ſeemed to me of any importance, and which deſtroyed any part of the received theories, I have repeated the ſame experiment twenty times and oftner, until I was convinced of the certitude and conſtancy of the phænomenon, taking great care not to declare a fact for truth, whereof I had the leaſt cauſe to doubt, For the ſake of order in this diſſertation, I intend to begin with the courſe of the arterial blood. The concurrence of ſentiments concerning the exiſtence of its motion, from the heart even to the extremities, would be ſufficient to confirm me that I am in no error by admitting the ſame: I have nevertheleſs added to this teſtimony that of experiments, and have made all that could be endeavoured, as much as if Harvey and Wallæus had made none. Being perſuaded that from hence we ſhould receive a double advantage; repeated experiments give new force to what great men have taught us; and I hope to diſcover ſome truths that have eſcaped them. This expectation is formed on that good intention nature hath been ſo kind to furniſh me with; ſhe is never conſulted in vain, and always makes ample amends for the pains they undergo who ſtudy her. I will begin with the motion of the blood, becauſe it may be obſerved without opening the veſſels.

Firſt, I am aſſured that the blood, propelled by the heart, dilates the arteries and creates what is called the pulſe: this phænomenon ſometimes is wanting in animals that have warm blood, perhaps becauſe the coldneſs of the external air, coagulates the blood in the veſſels. It hath been denied, that a pulſe hath been obſerved on the opening of living animals (*a*); I have nevertheleſs very often ſeen in cats, dogs, and ſheep, the pulſation of the arteries; they extend and ſenſibly affect the finger, on touching them at the time the heart contracts; if they are tied up their reſilition not only becomes more ſenſible, but they appear to lengthen themſelves, eſpecially on obſerving them attentively at the angle of any flexion, wherein is very diſtinctly ſeen, even where there are no ligatures, that the part of the artery the neareſt to the heart, lengthens itſelf; thereby making with the other branch, a more acute

---

(*a*) M. Stehelin Diſſert. de Pulſu, p. 9. teacheth us there was at Montpellier, a living dog without any ſenſible pulſe.

angle (*b*). The conical form of the arteries much contribute to the producing of this phænomenon; for it is for this reaſon, that the impulſe of the blood, againſt the parieties of the arteries, is (in proportion to the diſtance) ſo much the greater, as they are the more diſtant from the heart. Experiments made upon the arteries, and even upon very ſmall ramifications, as the Mammary, and others, have ſatisfied me, that they beat all at the ſame time, as alſo the coronary artery: although great men have told us, that this little artery is different from the others in this reſpect; but having opened it at different times, I always found that it ſpurts at the time of the heart's contraction, and that the blood runs ſlowly at the time of its relaxation (*c*).

The pulſation of the arteries become inſenſible, when they are of but one ſixth part of a line diameter; I have obſerved it upon the membranes of the Inteſtines of a live animal, at the laſt curvature of an artery, and in the ſuperior branch at the angles, but in the part of the artery beyond it was loſt.

(*b*) M. Weitſbrecht Comment. Acad. Petrop. tom. VII. p. 317, ſaith that all the artery is changed and diſplaced; and the celebrated M. Van Swieten hath obſerved the little arteries of a finger almoſt taken off, to lengthen themſelves at every pulſation.

(*c*) M. Starke, p. 26. relates the ſame experiment in his excellent diſſertation, De Reliquis Inſtrumentis quibus Sanguis in Circulum, &c. N° 22.

The author, who hath lately aſſerted (*d*) that an artery, ſo far from dilating itſelf, contracts at the time of the pulſation, will be convinced, by taking an example from the heart and the Penis, that a fluid propelled in a flexile canal, may render it longer and larger at the ſame time.

I have found the number of pulſations in a fixed time, much more frequent than is commonly ſuppoſed, in taking an account, whereof I have more than once mitigated the anxiety which my indiſpoſitions had produced. I have obſerved that in a morning, after the warmth occaſioned by being in bed was removed, my pulſe was a little leſs frequent than at night; the number of pulſations in a minute was from 70 to 80: and this number is proportionally leſſened, as a perſon is the leſs inclined to a fever; for in hypocondriacal caſes, with watchings and ſweating whole nights, the number of pulſations was only from 66 to 68. In general, in a phlegmatic perſon, we reckon 60 puſations a minute, in a lively perſon from 68 to 80; after a meal, the number is in-

---

(*d*) Otia Phyſiologica, p. 26; he hath only renewed the opinion of James Primroſe, the firſt adverſary of the circulation, who wrote poſitively that at that time of the ſyſtole, the artery became more elevated and narrower. He gives, at the ſame time, a figure of this change, according to his imagination. De Struct. Fundamentor. Plempii. 87.

creaſed by 10 or 12 pulſations a minute. Hence it is, that thoſe who are on the recovery from a dangerous illneſs, having often 90 pulſations in a minute, the increaſe which a meal procureth, raiſeth the number to 100 and 108, which is a feveriſh ſtate: when the time of ſleep comes on, the number of pulſations increaſe even to 80 and 84 (*e*). This is doubtleſs one of the cauſes of the increaſe of fevers at night; for 10 pulſations added to 110, which is often obſerved in an ordinary fever, make 120, which would produce a fever that could not be long endured.

In fevers that intermit, on the days of intermiſſion, there are generally obſerved 94 pulſations, and if there is ſomewhat of a fever 100; catarrhal fevers of the mildeſt ſort, have 110 and 118 to 120. In the increaſe of a Quotidian, there is generally obſerved 114, and in a violent ereſipelatous or milliary fevers, or in the paroxyſm of a Tertian, this number is increaſed to 130 or 134. Above this number it is almoſt impoſſible to diſtinguiſh; and there is only to be perceived a continual fluttering. The paroxyſm is always over, when there is obſerved not above 90 pulſations; and the danger of an acute fever is over, when the pulſations do not exceed that number. We

---

(*e*) Schwencke, p. 140.

cannot form any judgment of heat or ſweat by the pulſe; I have been in a ſweat without ſleeping, with 66 pulſations; I was very drouthy with 134; with the ſame number of 66, I have had a moderate heat without any ſweat; and with 100, to which the fever was reduced, I found myſelf well, and as a perſon that is neither hot nor cold. I have obſerved theſe ſame variations in a woman, of a conſtitution and age different from mine.

Give me leave to add the following phænomena concerning the pulſe: if a ligature is made on the artery, the pulſe ceaſeth in the part of the artery that is under the ligature. Fallopius made this experiment, and made uſe of it to prove, againſt the doctrine of the ſchools, that the pulſe did not depend on any force belonging to the artery (*f*). An Aneuriſme, in this reſpect, does not produce the effect of a ligature. I have often obſerved, in artificial Aneuriſms in frogs, that the motion of the blood appeared in reality ſlower in the tumour itſelf, yet the pulſe had its natural frequency under the Aneuriſme: this I have found by experiments on frogs. Harvey obſerved the pulſe under the Aneuriſme, but he ſaid it was more feeble (*g*). Modern phyſicians ſay, that the pulſe is produced by a wave of blood going

---

(*f*) De partibus Similaribus, p. 100.

(*g*) Diſſertat. de Circulo Sanguinis, tom. II. p. 215.

out of the heart, with more ſwiftneſs than that which preceded, and had loſt its force by the reſiſtance of the ſmall veſſels; and that the reſiſtance which this wave makes (*h*) againſt that which ſucceeds, is the cauſe that its direct courſe is impeded, and that a part of its force is exerted laterally againſt the parieties of the veſſels, and elevates them.

What perſuades me that the conical form is not the principal cauſe of the pulſe, is, that the pulſations appear ſenſible, and are even very ſtrong in the Carotids, which are not ſenſibly conical, and alſo are perceived in the very ſmall cylindrical veſſels. In frogs this force of the arterial blood, which produceth the ſyſtole of the heart (*i*), is not ſenſible, even by the microſcope, while the animal preſerves its ſtrength: but it is obſerved

---

(*h*) M. De Sauvage de Pulſu, p. 19. and his Comment. on M. Hales's Hæmaſtaticks, p. 279.

(*i*) Antony De Heide de Ven. Section. Experiment. p. 6. Henry Baker's Microſcope made eaſy, p. 136, and George Adams p. 45, have obſerved this acceleration. Lewenhoeck's Experiment. & Contemp. p. 167, hath denied that it was perceived in the ſmall veſſels; and the theory of Bryan Robinſon tends to the ſame negative opinion, Anim. Œconom. Propoſ. 11. It is eaſy to account for theſe two different ſentiments, by a very natural ſuppoſition, which is, that theſe obſervations were made at different times; Lewenhoeck made his obſervation when the animal was in full vigour, and the others when the ſtrength was exhauſted. What confirms me in this idea, is that Lewenhoeck himſelf has deſcribed this phænomenon in another place. Experim. & Cont. t. II. p. 175 & t. III. p. 114.

when the animal begins to grow weak, at which time we may diſtinctly perceive, that a freſh wave that comes from the heart, propels and drives forward that which preceded. The eye which cannot perceive the difference of 1001 and 1000, may diſtinguiſh very well the difference of 11 and 10, although both the one and other differ only in 1: after the pulſation, the artery remains equally full; and although its diameter decreaſeth, yet it appears not to empty itſelf.

It is not ſufficient for the formation of the pulſe, that the heart drives the blood into the artery; but the artery muſt be capable of extenſion, ſo as to give way to this preſſure: if it is too ſtrong, the motion of the blood will take place, but it will be exactly as in a glaſs tube, and without any elevation of the parieties. In frogs the Aorta, the pulmonary artery, and the great veſſels of the arm, have manifeſt pulſations; but the deſcending Aorta and largeſt trunks of the Meſentery have none, for their membranes are too ſtrong; and it is impoſſible they ſhould give way to the impulſe, which the heart can impart to the blood: this I have ſeen one thouſand times; and I may at any time (*k*)

(*k*) M. De Sauvage Pulſ. Theor. p. 24. to the truth of theſe experiments; and my pupil M. Remus p. 48, have reported this want of pulſe in frogs, perhaps after my obſervations.

make the ſame obſervation with the ſame reſult. I have very often ſeen a vein ſo to lie on an artery, as to divide its ſmalleſt motions, yet it was not at all moved at the pulſation of the artery. Arteries that are oſſified, have no pulſation (*l*); and nature hath been very prudent in preventing the arteries of frogs, which do not contract themſelves, from being ſuſceptible of dilation.

A ſecond law of the motion of the blood in the arteries, which may be obſerved without opening them, is, that it is carried from the heart to all the extremities: this is proved by ligatures. I have repeated Dr. Harvey's experiments, I have put ligatures on the greater part of the arteries, and firſt on the Aorta, which I have very frequently tied at a ſmall diſtance from its going off from the heart, in eels, frogs, dogs, cats, and other animals. It is aſtoniſhing to obſerve how much it ſwells between the ligature and the heart; it becomes of a brilliant red colour, and ſenſibly lengthens itſelf at each pulſation, during which time the heart is violently agitated by the continual irritation of the blood, whereof it endeavours in vain to diſcharge itſelf; this ligature is one of the cauſes which ſuſtains the motion of the left ventricle, very long after that of the right is loſt, which I have fully ſpoken of elſewhere (*m*). When the

---

(*l*) M. Senac, tom. II. p. 225.

(*m*) Commentar. Societ. Regiæ Gotting. tom. I. p. 263. This memoir is reprinted and follows this.

artery under the ligature is touched, no pulſation is obſerved, and when it is opened, gives no blood (*n*).

I afterwards put a ligature on the pulmonary artery, which is not done without much difficulty, when I obſerved the ſame phænomena as before on tying up the Aorta, the right ventrical extremely full and extremely agitated, and the pulmonary artery exceſſively ſwelled, emitted its blood on being opened, with as much force as the Aorta itſelf. Theſe experiments agree but little with what a certain geometrical phyſician advanceth more than once (*o*), which is, that ligatures make the arteries ſwell leſs than the veins, and that the ſwelling of the arteries, produced by ligatures, is almoſt inſenſible; yet it is certain that the Aorta of a frog, which is not larger than one of the ſmall arteries of the human body, which anatomiſts hardly take any notice of, ſwells prodigiouſly when it is tied up or compreſſed, and that the experiment may ſucceed, as M. de Sauvage hath deſcribed it. The ligature muſt be made upon an artery, that its trunk may diſcharge itſelf into ſome branch above this ligature; and the ligature muſt be made only on very ſmall arteries: if for example, ligatures are made on the meſenteric arteries of a frog,

---

(*n*) Drelincourt Canicid. 1.

(*o*) Theoria Tumor. p. 19. Pulſus Theor. p. 26.

which are only capillary veſſels in a dog, the blood immediately becomes immoveable in that branch that is tied up, without any ſwelling; preſently after it becomes retrogade in the neighbouring ramifications, and leaves its veſſel intirely empty even to the ligature, and filled underneath by the blood that was there, whoſe motion the ligature had ſtopped. This ſingular fact ſeems to depend (*p*) on the hardneſs of the artery, which the force of the heart in this animal could not overcome; and on the motion of attraction, of which I ſhall ſpeak in another place; and alſo on the facility that the blood finds in paſſing from one branch to another. I muſt moreover remark with M. de Sauvage, that the blood doth not always make ſuch efforts againſt the obſtructed veſſels, as ſome phyſiologiſts have imagined (*q*), but it paſſeth inſenſibly and eaſily into the neighbouring veſſels, which it dilates ſucceſſively: hence the veſſels of the Pelvis become very large, after a ligature is made on the umbilical arteries.

When a ligature is made on an artery in animals which have warm blood, a conſiderable tumour forms itſelf above the ligature,

---

(*p*) M. Remus hath been a witneſs of this as well as myſelf, p. 49, 50.

(*q*) Cheſelden before cited. p. 203. Hoffman de Elaſticit. Fibra. p. 9, 10, where he reports an experiment on frogs which makes againſt him, the celebrated M. Senac, t. II. p. 172.

which nevertheleſs diminiſheth gradatim; although the ligature remains on, which afterwards becomes changed into a ligament, whoſe internal ſurface is filled with a filamentous white ſubſtance: the ſame happens in the umbilical arteries. In arteries that are obſtructed, even in the Carotid, according to Mr. Emet's experiment (*r*). I cannot ſee by all theſe facts, how M. de Sauvage could conclude, that a ligature creates a leſs tumour in the arteries than in the veins; for the experiment on the Aorta of frogs, which I have abovementioned, ſufficiently proves the contrary; and the ſwiftneſs of the arterial blood was a ſufficient proof at firſt ſight, and the more ſo, becauſe the veins have a greater number of ramifications for receiving their blood.

I now return to my experiments. When a ligature is made on an artery in the lower belly, it ſwells and hath a pulſation above the ligature; it emptieth itſelf alſo above, and when it is opened in this place, it emits no blood: the animal, as Steno hath already obſerved, looſeth the motion of his legs, cannot ſtand on his feet, and moves them after him, as if he dragged ſome heavy weight. I have ſometimes obſerved convulſions in theſe parts; I have repeated, and mention here this experiment, becauſe it had been doubted by

---

(*r*) Teutamen. 2. p. 27.

great men (*s*): it does not ſucceed in frogs; and although the Aorta is tied up or divided, they can leap and make their eſcape. A ligature on the thoracic Aorta, produceth in a cat the moſt violent ſymptoms; the whole animal becomes ſtupid and inſenſible. When a ligature is made on the Carotid in live animals, or the meſenterick artery, or the Crurale or Brachial, every where the ſame phænomena appear. A tumour above the ligature, which the congeſtion of blood renders of a ſhining colour, there is a pulſation, and the artery lengthens itſelf at the time of the ſyſtole of the heart, and diminiſheth in the Diaſtole, the motion of the legs or the other members, undergo no alteration (*t*); and a ligature, even on the Carotid, hath not created any conſiderable change. What I have hitherto related may be ſufficient to teach us, that the blood paſſeth from the heart into the Aorta, and from the Aorta into all the arteries of the whole body. I muſt at preſent ſpeak of thoſe motions of the blood that are leſs apparent, and which are made in a more obſcure manner, and alſo which are not perceived until the veſſels are opened, unleſs their membranes are tranſparent, as they generally are in frogs. On

---

(*s*) Swamnerdam Biblia Nat. p. 850, and author of a new memoir on the motion of the muſcles, preſented to the academy of Berlin, N° 20.

(*t*) Drelincourt Canicid. 9.

theſe animals chiefly and on fiſh, I have obſerved theſe motions, and will now ſet forth exactly what appeared to me.

The firſt phænomenon that offers itſelf, is the rapidity with which the globules of blood move from the heart to the extremities, as I have obſerved it in the Meſentery of a frog, in the membranes for the ſpawn, and in the tails of fiſh: it is very difficult to eſtabliſh any proportion between the ſpace it had paſſed, and the time employed in paſſing, becauſe the ſpace that is circumſcribed by a microſcope is ſo ſmall, that the time which the blood muſt employ in paſſing, is leſs than any ſenſible meaſure. The inequality of ſwiftneſs in the motion of the blood in animals that have cold blood, ſeems to be the reaſon that perſuaded Dr. Hales (*u*), that its motion was forty-three times more ſwift in the lungs of a frog, than in the muſcles. As for my part, I have not obſerved that it moved in the lungs with any greater velocity, than in the Meſentery; but I have very diſtinctly ſeen, that it had leſs velocity by far, than the blood that ſpurts from an artery that is opened. I cannot ſee what ſhould occaſion this very great ſwiftneſs in the lungs of frogs: it is true, that at firſt ſight we very diſtinctly ſee in theſe animals, that at the time when the lungs are expanded, their principal artery,

---

(*u*) Hæmorſtat. p. 68.

which extends itſelf their whole length, and which ramifies from one part to another, becomes almoſt ſtraight, and makes a very eaſy paſſage for the blood; but at the time when this viſcus ſubſides, as in expiration, the artery is found contorted in a ſerpentine manner, making the paſſage for the blood more difficult. But this particular quality of the lungs, is no proof againſt the velocity of the blood in other parts, in which it, at all times, finds the ſame eaſe for its circulation: moreover it frequently happens, that the blood circulates with a much greater velocity in an artery of the Meſentery, when it moves very ſlow, or is even intirely at reſt in the others. Hence we may obſerve, there is nothing extraordinary in thoſe topical fevers and heat incident to any limb: I have moreover obſerved, that the blood was much leſs retarded in the ſmall veſſels, than the geometrical phyſicians imagine. In frogs it is impoſſible to diſtinguiſh any difference between the velocity that is obſerved in the ramifications of the arteries, and that which is obſerved in the trunks; and it ſeems to have the ſame velocity in the largeſt meſenteric veſſels, as in its laſt viſible ſubdiviſion. I have ſeen in a dog, which is an animal that hath warm blood, this fluid to ſpring from a ſmall ramification of the mammaries, of leſs than half a line diameter, to the diſtance of ſix feet and an half; and that in another dog, of which Keill opened the Iliac artery, it ſprung not above one half of this diſtance (*x*). There is alſo a much greater velocity in the ſmall veſſels of fiſh, as I ſhall prove by an experiment, which I ſhall relate in ſpeaking of the veins.

If the velocity of the blood in frogs was the ſame as in man, according to Keill's calculations (*y*), which are, that in the veſſels of one globule diameter, it ſhould go in a minute only the $\frac{75}{5225}$ of an Engliſh foot; but I have often ſeen, in the ſmall meſenteric veſſels of a frog, that the velocity was ſo great, that I could hardly follow it with my eye, and that ſometimes it equalled, and at other times exceeded that of the blood in the great veſſels; although, according to theſe calculations, it ſhould have been 1448 times ſlower (*z*). But if this velocity is ſo conſiderable in the ſmall veins, it muſt be more ſo in the ſmall arteries from which theſe veins have their origin and motion, and more conſiderable yet in the arteries that are larger than the capillary ones. I have even ſeen veins, of one or two globules diameter, to carry their blood with a ſufficient velocity, though in the large arteries it was very ſmall; and indeed

---

(*x*) De Vi Cordis, p. 41. t. I. fol. 5. the Holland edit.

(*y*) De Velocitate Sanguinis, p. 36.

(*z*) De Secretione Animal, p. 56, the ſame edition:

what the more takes away all cauſe for doubts is, that in obſerving a branch which aroſe from a trunk much larger, and which returned up again the length of this ſame trunk, I obſerved that the blood had a much greater velocity in the branch than in the trunk. I have not had in view, in repeating theſe experiments, any intention of publiſhing what errors there may be in the calculations of great men, who have treated of theſe matters; I only ſet out honeſtly facts, which are proofs very ſuperior to theories, and which ſhew us, that the velocity is very great in the ſmall veſſels; other experiments eſtabliſh the velocity, by which the perſpiring veſſels emit their vapour. Accurate obſervations will prove the truth of what I am going to relate, concerning the motion of the arterial blood. The red globules ſwim equally diſtributed in the ſeroſity without any confuſion, and are moved in right parallel lines, without rubbing or mixing themſelves together, and without any manner of rotation (*a*); ſo that all that hath been reported concerning the vortical confuſed motions of the blood, hath no manner of foundation, with reſpect to animals that have cold blood; for the experiments which I have made, in order to obſerve its motion in thoſe animals that have warm blood, have not ſucceeded. The preſſure of the

---

(*a*) G. E. Remus, p. 37, 43, &c.

the blood againſt the parieties of the veſſels, and againſt the reſiſtances it meets with at the diviſions of the arteries, is not very great; it is eaſy without any repulſion, and it is very far from dividing and reſolving the globules, or changing their figure: and it is ſo eaſy, that it diſcompoſeth not the weak covering of the air which forms the bullæ, which are frequently ſeen in the veſſels of frogs: when the progreſſive motion of the blood is ſtopped, the globules become immoveable, and remain always at reſt until the cauſe is removed. Hence we ſee that eminent men (*b*), who have ſuppoſed in the blood an inteſtinal motion, capable of contributing to its progreſſive motion, are quite deceived. I have repeated this experiment ſo often and with ſuch conſtant ſucceſs, that I am perſuaded nothing can ever be alledged againſt me; I have often carefully obſerved if I could perceive any difference between the velocity of different globules in the ſame artery: it ſeemed to me, that thoſe which were in the center, and which moved in the Axis of the veſſel, have a greater velocity than thoſe which touch the parieties; and this obſervation is conſonant to thoſe of Malpighy (*c*) and Schreiber (*d*).

(*b*) B. Stevenſon in the Eſſays of Edinburg, tom. V. p. 2.

(*c*) Poſthum. p. 92.

(*d*) Elementa Medic. p. 323.

When an artery divides itſelf, the globules, if there is no diſorder in the circulation, divide themſelves proportionally between the two branches; and I have carefully obſerved, if in the angles of the veſſels, there was any difference in the velocity of the blood in coming to theſe angles. I have obſerved in frogs, different branches ariſing from trunks under different angles; and when I found no natural angles and curves, I made ſome. This is the reſult, ſuch as I find by experiments; for I have not repeated them often enough to be aſſured, that nothing remains for diſcovery on this article, this obſervation being attended with much difficulty.

The firſt artery that I obſerved, divided itſelf into four branches; two went off from the trunk in a right angle, the two others ſeparated a little from the direction of the ſuperior trunk, with which they formed very acute angles. I obſerved for near ſix hours, that the branches which went off in a right angle, diſcontinued their motion much ſooner than the two others; a ſecond artery divided itſelf in a different manner; the more conſiderable branch went off but little from the direction of the trunk; the other much ſmaller formed a conſiderable angle with the trunk. I could not immediately diſtinguiſh any difference in the velocity in either of theſe veſſels; but when the motion of the blood began to abate, I obſerved that it continued much longer in thoſe branches that went off in the leaſt angle from the direction of the trunk. It appears by this obſervation, that the velocity in the branches is ſo much the greater, the leſs the angle is they make with the trunk; and this is found conſonant to the theory of M. M. de Sauvage (*e*), Senac (*f*), and Hales (*g*), M. Remus (*h*), hath made theſe experiments with me. The event is not always the ſame; and I have often obſerved the motion to ceaſe in an artery, and yet continue in a branch that went off from it in a right angle.

I much deſired to know what was in a living animal, the effect of curvatures in the veſſels; I knew very well, that they have much influence on the injections that are made on a dead ſubject; and that it is ſufficient to bring back an arm upon the body, whereby an angle is made at the ſubclavian artery, that the injection may fail in this member, becauſe of the difficulty that it finds in entering it. In order to ſatisfy myſelf on this article, I detached an artery from its cellular membrane that connected it to the Meſentery,

---

(*e*) Diſſertation ſur les medicaments qui affectent certaines parties du corps, p. 26.

(*f*) Tom. II. p. 167.

(*g*) Hæmaſtaticks, p. 67, hath given experiments hereupon.

(*h*) Page 43.

and turned it ſide-ways, ſo as to make it form a conſiderable curve: but I could not obſerve, that the velocity of the blood was thereby diminiſhed (*i*). I took another artery and turned it to a point, ſo that it made a very acute angle, with that part of the trunk that was the neareſt the heart; but this flexion did not yet diminiſh the velocity of the blood; from whence I conclude, as I ſhall ſay preſently, that the force of the heart is ſtrong enough to overcome, without any ſenſible diminution of its power, any reſiſtances of this kind: but I ſpeak with reſpect only to ſimple flexions, and not of thoſe which are ſo complex as in the Epididymis. The whole world agrees, that theſe much retard the courſe of the Semen: in order to be convinced thereof, we need only conſider how ſlowly mercury paſſeth them, and alſo a little reflect how much force any fluid muſt loſe in overcoming obſtacles ſo long continued.

Laſtly, in frequently obſerving firſt natural Aneuriſms, and afterwards thoſe which I created myſelf, I began diſtinctly to diſtinguiſh the change of motion in a fluid paſſing from a narrow canal into a veſſel that is larger, like that which happens to the water of any river that diſchargeth itſelf into a lake or pool. The velocity of the blood dimi-

---

(*i*) Theory made M. Micholotte thus judge of this matter, De Secret. Fluidor. p. 139, 140.

niſheth ſenſibly in the cavities of Aneuriſms, and circulates through them but ſlowly, which may give riſe to a re-union of the globules; and a ſimple coagulum may be formed by the connection of many globules together, and which the force of the heart may reſolve, by putting into motion the diſunited globules. It will be more ſurpriſing to find, that under the Aneuriſme, the blood reſumes its former velocity, and moves with as much rapidity as before it entered the ſame; and though the blood moves very ſlowly in the Aneuriſme, the propelling force, which ſeems almoſt exhauſted in this part, is renewed, whereby the blood receives its natural velocity. It is not ſeldom that we obſerve the blood to move with a greater velocity in a ramification, than in the trunk from which it ariſeth.

This experiment, which I have often repeated, ſeems to me very important, and in a manner radically diſproves all that hath been aſſerted with ſo much gloſs, concerning the pretended diminution of the velocity of the blood in the ſmall veſſels (*k*).

It ſeems that in animals the power of the heart is much ſuperior to the reſiſtance that

---

(*k*) Keill eſtabliſheth the Ratio between the velocity of the blood in the Aorta, and of that in the capillary veſſels, as 5233:1, Robinſon as 1100: 1, Butler as 500: 1; but the celebrated Dr. Hales hath not taken notice, that the motion of the blood was very ſlow in the capillary veſſels.

can ariſe from the ſmallneſs of the veſſels; and it always happens in that caſe, that the velocity is increaſed in canals, in proportion to the diminution of their diameter. We have an example hereof in fountains, whereof the water riſeth with greater force the narrower their tubes are, as far as to the point where the force of adheſion, between the fluid and the parietics of the tube, becomes ſtronger than that of the impulſion, which is a caſe particular, and has nothing to do in this place.

The theoreme which I have now eſtabliſhed, wants not much demonſtration. If the caſe was otherwiſe, the minuteneſs of the veſſels would diminiſh the velocity of the fluids; if the force of the frictions produced a decreaſed velocity in proportion to the diminution of diameters, in that caſe it muſt happen, that the blood, in paſſing from an Aneuriſme, or from any large veſſel into another that is narrower, would diminiſh its velocity; but by obſervation we may obſerve the contrary, as I have before related. tI will perhaps be an objection to me, that I have in my obſervations made uſe of a microſcope, which makes the velocity of the blood ſeem greater: but this objection cannot avail, becauſe I have uſed a microſcope in obſerving the great trunks as well as the ſmall veſſels; therefore the increaſed velocity being the ſame, ſimilitudes are not changed, by which only, as to this matter, we are to form a judgment.

I have been deſirous to obſerve what happens in the Anaſtomoſis's, and at the re-union of two veſſels meeting by oppoſite directions. I have found that in this caſe the two currents reciprocally reſiſt each other, and that the globules of the one ſtrike againſt thoſe of the other, until the weaker current gives way and is overcome by the ſtronger: the ſame nearly happens with the blood flowing from an artery that is divided, and which generally runs by two oppoſite currents. If it flows from ſome branch of an Anaſtomoſis, or from a flexion of an artery, it is to be preſumed that the two currents will meet there, as the two currents of an artery that is wounded meet and flow out of the wound. From hence it may be concluded, that in the human body, where there are very frequent Anaſtomoſis's, the blood may be moved in different directions, according as one of the branches, communicating with the other, ſhall have a greater or leſs velocity than its companion. As for example, in the arteries that are found between the back and the palm of the hand, and which I have called perforantes ſuperiores (*l*), it is evident, according to the ſituation of the hand, that the weight of the blood will determine its

---

(*l*) Iconum Anatomicarum Faſcic. 6. p. 41, 42.

courſe, either from the palm to the back of the hand, or from the back to the palm. I will ſet forth in another chapter the power of this weight, which will be found one of the cauſes of the motion of the blood.

It appears that the great deſign of nature, in thus multiplying the arterial circles and the Anaſtomoſis's, hath been in a manner for this purpoſe: viz. if a trunk ſhould be obſtructed, deſtroyed, or obliterated, its branches may receive blood from the neighbourly trunks. This I have with ſatisfaction obſerved in human ſubjects, wherein I have found a carotid, a vertebral, or a brachial artery, obſtructed, oſſified, and uſeleſs, and yet the circulation not diſcompoſed.

I muſt now ſpeak of the irregularities that happen in the motion of the blood; ſometimes it is retarded and troubled, ſometimes it ſeems obſtructed by ſomewhat it is endeavouring to overcome; ſometimes it is retrograde, ſometimes at reſt, and the veſſels become empty.

The retardation of the blood, is generally the firſt of all theſe irregularities.

The velocity is troubled when it is in general diminiſhed, and all of a ſudden a new effort of the heart renews the velocity, which in a moment after it looſeth again (*m*). In-

---

(*m*) A. Van Lewenhoeck hath obſerved ſomewhat like this. Experim. & Contemp. p. 159, 165, 179, &c. G. Adams, l. c. p. 45.

anition is generally joined to this ſlowneſs of motion; and generally few globules are obſerved in a veſſel wherein this lentor predominates. Always when the motion of the blood recovers itſelf, after any diminution or retrogradation, we diſtinctly ſee every wave make a reſiſtance to that which follows it, and that this laſt muſt conſequently impart to the other its natural motion. Hence we ſee, that this reſiſtance, which one wave creates to another, ought to be taken notice of in the general reſiſtance which the heart meets with, and it ought not to be neglected when we would make an eſtimation of it (*n*).

Oſcillation is almoſt a conſtant effect of the retardation of the arterial motion; in this caſe the blood goes and comes, and alternately it purſues its natural courſe, and becomes afterwards retrograde (*o*).

This fluctuation is very ſingular in the places where the artery widens itſelf; ſometimes the blood of one of the branches in its retrogradation, makes a reſiſtance to the blood of the trunk, which being of ſuperior force, repels it either into its own proper branch, or into ſome other; from whence it returns ſome little time after it had been at reſt, to be

---

(*n*) Morriſon Lettres, p. 26.

(*o*) This motion of Oſcillation is deſcribed in Lewenhoeck. Experim. & Contemp. p. 164, 165, 186, 188. tom. iii. p. 111, 112; and in Boerhaave De Uſu ratiocinii Mechan. p. 34: ſee alſo F. W. Horch. L. c. p. 115.

repelled again as before (*p*): at other times one of the branches, capable of making a greater reſiſtance, forceth its blood to run back into another, either croſs the trunk, or upwards towards the heart by the ſame trunk.

I have ſeen in a branch, which went off from the trunk under a very great angle, that the blood became retrograde with ſo great a force, that after ſome oſcillations, it re-eſtabliſhed the natural courſe of the blood, in the part of the trunk that was inferior to the riſe of the branch. This produced a freſh oſcillation between the branch, and the ſuperior trunk, in which there were but few globules, and the trunk by this oſcillation recovered its natural motion; ſo that the trunk and the branch formed two ſources, which conveyed the blood into the inferior trunk as into a common reſervoir. This continued ſome time; and this confuſion terminated at laſt, by the ceſſation of the retrograde motion of the branch, and the ſuperior trunk diſcharged its blood into its two diviſions, as in the natural ſtate: but before this was compleated, there were obſerved, deſcending from the ſuperior trunk, clouds formed by collections of the globules; and this obſtacle having been overcome, all was reſtored to their natural ſtate. I have alſo obſerved a remarkable oſcillation,

---

(*p*) M. Miles hath obſerved the ſame. See the Tranſ. Phil. vol. 41. No 460. p. 728.

between the coagulated blood in an Aneuriſme, and the blood of part of the artery which had yet preſerved its motion; ſometimes the globules of the Aneuriſme gave way to the blood of the artery, and the next moment the globules repelled it, and forced it to paſs into other branches that made no reſiſtance. What is moſt particular in this oſcillation is, that the globules of the arterial blood were repulſed by an inviſible fluid, before they came into contact with the red blood of the Aneuriſme. Theſe oſcillations, with which I have ſo often entertained myſelf, terminate and end either by the re-eſtabliſhment of a natural ſtate (*q*), when the force and power of the heart begins to prevail, or by an intire retrogreſſion or a ceſſation of motion, which is known by the longer diſtance of the intervals of theſe oſcillations.

It often happens, that the power of the heart re-eſtabliſhes the natural motion of the blood; and the reſolution of the grumes of blood, produced by theſe oſcillations, diſcover to us one of the advantages ariſing from Anaſtomoſis's, which is to overcome obſtructions ariſing by the blood which an artery, void of any obſtructions, propels againſt that blood that is beginning to be at reſt.

(*q*) A. Lewenhoeck Experim. & Contemp. tom. ii. p. 164, 165, tom. iii. p. 112.

Retroceſſions are pretty frequent; they are oftener ſeen in the veins or arteries after the heart is divided, and are obſerved, though the power of this muſcle begins to diminiſh, and though the circulation begins to abate. At this time the blood of the branches or ramifications go back into the trunks, and from thence towards the heart; ſometimes it happens, that its progreſſive motion is performed regularly in ſome branches, although in others it is retrograde: I have obſerved this both in fiſh as well as frogs. Very large veſſels are ſubject to this irregularity, at a time when the motion was natural in the ſmall ones; I have obſerved two branches of the ſame trunk, the one carried its blood forward, in the other it moved in a contrary direction, and was repelled from the branch into the trunk; I took notice, that commonly the cauſe of this retroceſſion, was ſome obſtruction, as an Aneuriſme, in which the coagulated blood makes a reſiſtance to that coming from the arteries, and compels it to a retrogradation: this preternatural motion does not ceaſe until the grumes of blood that occaſioned it have been carried off. Laſtly, retrogradations take place after large wounds, and eſpecially after the amputation of the heart and in the arteries, wherein the motion of the blood ceaſeth. M. Senac (*r*) hath taken no-

---

(*r*) Traitè du Cœur, tom. ii. p. 174.

tice, that fainting is one of the cauſes of this retrogradation of the blood.

Theſe frequent retrogradations deceivedold Mr. Lewenhoeck, who was a diligent obſerver, yet a man of litttle learning; he confounded the veins with the arteries, and aſcribed, many years before his death, the acceleration and pulſation (s), and alſo the blood's courſe towards the heart to the venal blood; and aſcribed to the arteries its ſlowneſs, reſt, and return towards this muſcle.

The ceſſation of motion puts an end to all; and this ceſſation always remains in the arteries of frogs, when any artery hath diſcharged all its blood, or when it receives no more from the Aorta.

This caſe I have often ſeen in frogs, not only in thoſe which had faſted a long while, but alſo in thoſe that had been well fed; their arteries are ſometimes found intirely empty, and ſeem like white nerves, with which indeed Lewenhoeck hath confounded them (t). This phænomenon perhaps is the cauſe that deceived the ancients, who imagined that the arteries contained only air: it is very certain that they are ſometimes intirely empty, although eminent men have been of another opinion; ſometimes it happens that the blood is at reſt in ſome branches, when it is in motion in others: that is to ſay the ſmall veſſels; for theſe preſerve their motion after the trunks have loſt theirs.

(s) Epiſtol. Phyſiologic. tom. IV. p. 167. Philoſoph. Tranſact. N° 319. V. Uffenbach. Reiſen, tom. iii. p. 350.

(t) T. ii. Epiſtle 119. p. 112.

Alſo it frequently happens, that when the animal becomes languid, the blood ſtops its motion almoſt at the beginning of the Aorta, ſo that the veſſels more diſtant from the heart, are not ſupplied with blood; ſometimes the arteries empty themſelves, and the motion of the blood abates, ſo that at laſt they become intirely empty. Somewhat like this happens in man, in extremity of cold, with loſs of pulſe; ſometimes the want of motion in the arterial blood is joined to their inanition, when there is but few or no globules in the artery: nevertheleſs we frequently obſerve a ſmall number of globules, which although diſtant the one from the other, yet continue their motion.

When the motion of the arterial blood ceaſeth, before the ſtrength of the body is exhauſted, which is known by the continuation of the motion of the venal blood, there is ſome hopes that the motion will recover itſelf without aſſiſtance, or by opening a vein. This recovery of motion begins by a ſmall number of globules which return into the empty veſ-

---

(*u*) Keill's quantity of blood, p. 91, 92.

ſel; in a ſingle row their number increaſeth by degrees; they dilate the artery which, as it becomes filled with blood, from a pale colour it becomes extremely red, and the blood at laſt moves therein with much rapidity.

I have often ſeen (*x*), as hath alſo Mr. Lewenhoeck, this recovery of the motion of the artery. We have frequent examples, that arteries filled with blood have loſt their motion, which accident hath been eaſily cured, either by bleeding or without any aſſiſtance from art, by the power and effort of the heart only. It often happens in this caſe, that the grumes of blood formed by globules connected together, are the firſt that put themſelves into motion, whereupon all the blood retakes its courſe; but we are not always ſo happy. It is always the caſe after death, that the arteries empty themſelves by degrees, until at laſt they become abſolutely white without the leaſt remains of blood, and become ſo like the membranes of the Meſentery, that it is very difficult to diſtinguiſh the one from the other. After death the motion of the arterial blood is not intirely at an end, until the Meſentery becomes intirely dry, and the globules themſelves begin to form dry maſſes. In animals that have warm blood, it can never be expected that the motions of

---

(*x*) Epiſtle 129. p. 112.

the machine can recover and re-eſtabliſh themſelves, when once the fat becomes fixed.

CHAP. IV.

The Motion of the Venal Blood.

I Come now to the motion of the venal blood, in treating whereof I ſhall purſue the ſame order that I obſerved in ſpeaking of the motion of the arterial blood. I will begin with the pulſe, which is generally believed wanting in the veins; yet I have frequently found a pulſe in the large veins of animals that have warm blood. There appeared in 1750, a diſſertation preſented to the royal academy of ſciences at Paris, the author whereof, M. Scheighting of Amſterdam, argues for the motion of the brain, againſt people he calls Sophiſts; I was reſolved to examine into this motion, which I did not in the leaſt believe, becauſe of the adheſion of the Dura mater to the Cranium; for this purpoſe, I made a great number of experiments on animals with M. Walſtorf, an able phyſician of Heidelberg, who ſtudied at that time at Gottingen; we eaſily perceived theſe motions, and that they correſponded to thoſe of the lungs, ſo that the brain ſwelled at the time of expiration, and ſubſided at the time of inſpiration. I enquired into the cauſe of this phænomenon; I ſuſpected that it depended on the facility that the blood finds in inſpiration, in paſſing from the right ear into the pulmonary artery, and from the neighbouring veins into this ear. To ſatisfy myſelf with reſpect to the truth of this conjecture, I determined to make ſome freſh experiments; I laid bare different veins in a living animal, eſpecially the jugulars, the Brachial veins, the Iliacks, and both the Cavæ, I eaſily perceived they were regularly filled at the time of expiration, becoming then ſwelled and red by the quantity of blood filling them, and that at the time of inſpiration they lengthened themſelves, became thin, pale, and empty, and gave no blood on being opened at this time: this I call the venal pulſe. I communicated theſe obſervations to my friends at France, particularly to M. de Raumur and to M. de Sauvage, from whom M. Lamure muſt have heard of my experiments: his own words are ſufficiently convincing, by the objections (*a*) he ſets up againſt me without naming me (*b*), and by letters which I ſhall quote. It is concerning this motion of the veins, that I ſhall now ſpeak, without

---

(*a*) Memoirs of the acad. royal of ſciences, 1749. p. 642.

(*b*) p. 656.

without knowing what paſſed at Montpellier, with reſpect to an eſſay that I read at the aſſembly of the ſociety of Gottingen, the 22d of April 1752, and which was printed in the memoirs of the ſociety for this year (c), a little time after M. Walſtorf communicated to the public, his experiments as well as mine in an excellent diſſertation (d). In this interval of time, M. Lamure ſent his experiments and thoughts on this motion, to the royal academy of ſciences at Paris, in a memoir which was read at one of the aſſemblies the 12th of Auguſt 1752, four months after mine, and which was printed in the memoirs of the year 1749. Some months after the publication of the memoirs of Gottingen, I imagined I ought candidly to report theſe facts without animoſity, in order to prove that I wrote concerning this motion of the veins before M. Lamure (e), and that my experiments have not received any advantage from his. My experiments are much more numerous than thoſe of M. Lamure, and contain many things that I have not found in his memoir, or which are therein contained in a different manner; and yet M. Lamure's memoir contain ſome facts, which I had not at that time obſerved. As for example, I was ſurpriſed at his good fortune, in having obſerved the motion of the brain, without detaching the Dura mater from the Cranium, but I have never ſucceeded in this caſe (*f*). Secondly, I have obſerved an alternate motion in the veins of the arm; I have ſeen another reflux of blood of both the Cavæ, produced by the venal contraction, which I ſhall ſpeak of in another place. I have obſerved a motion produced by the contraction of the Diaphragme; alſo the inſenſibility of the Dura mater, and other things which are found different in the memoir of M. Lamure; but I have never obſerved that a ligature, on the jugular veins of a dog, brings on ſleep (*g*), or that the ſinus's of the brain had any pulſe (*h*); and the exiſtence of a ſpace filled with air between the lungs, is diſproved by ſo many experiments, that it ſeems it never will be confirmed by any pe-

---

(c) Tom. II. p. 127.

(d) Diſſert. qua experimenta circa motum cerebri cerebelli, duræ matris & venerarum in vivis animalibus inſtituta propoſuit Gotting. Mens Mart. 1753.

(e) This is what the celebrated M. de Sauvage hath remarked in a letter dated March 1, 1752, but which was detained in coming, as follows. This dog was trepanned; we obſerved much the motion of the brain, very conformable to that account you have honoured me with, in order to inform me whether the reflux of the blood, during the time of expiration, is the cauſe of this elevation. M. Lamure hath opened above ten dogs; and indeed we have found the ſame phænomenon as you, and are very much obliged to you for this diſcovery: the original hereof I have by me.

(*f*) Walſtorf. p. 42, 43, 65.
(*g*) Memoirs of 1749, p. 543, 544.
(*h*) Ibid. p. 547.

netrating wounds of the breaſt made without wounding the lungs. A weak and vague experiment, on which M. Lamure much relieth (*i*).

This ingenious gentleman hath indeed diſcovered one of the reaſons why the jugulars and other veins ſwell at the time of expiration, which is, becauſe at this time the compreſſion of the breaſt in general makes the blood flow back from the Cava into the jugulars. I have very lately verified this experiment on a hog (*k*); and, by compreſſing a long time the Thorax, I have drove back its blood, ſo as to make the brain ſwell (*l*). This it was that made me of an opinion, that in expiration, the compreſſion of the breaſt much contributes to the filling of the veins: not that I would exclude the great facility the blood finds in entering the lungs at the time of inſpiration, and which, in this ſtate, takes away the ſwelling of the veins; nor the other motions which ariſe from the Diaphragm.

Many facts prove the veins empty themſelves at the time of inſpiration, by reaſon of the facility it finds in entering the lungs, and that they fill themſelves in expiration, at which time the blood with more difficulty enters

---

(*i*) Memoirs of 1749, p. 558.

(*k*) Ibid. p. 556. 562.

(*l*) See the Diſſertation of M. Walſtorf. p. 39.

enters this Viſcus. Hook's experiments, which I have often verified, prove that in lungs that had ſubſided and become unpaſſable. By inſpiration, the blood which had ceaſed to enter them, and which was not put into motion by the heart, now again enters them, and performs its courſe.

Injections even ſucceed better, and paſs eaſier from the pulmonary artery to the vein, when the lungs are inflated. From theſe obſervations this conſequence may be drawn, which is, that, without the action of inſpiration, the blood would with difficulty enter the lungs; and that without expiration, it would ceaſe to move in the veins.

Straining is no more than a retention of the air, and a retarding of expiration. The veins all ſwell in this interval, and the face becomes ſwelled and bloated; no doubt, becauſe the blood of the lungs is not expelled, as uſual, by the action of expiration: and, indeed, without this mechanical compreſſion of the breaſt, expiration may produce a ſwelling of the veins, by making a reſiſtance to the paſſage of the pulmonary artery into the vein, and into the left ventricle. But to theſe cauſes of the ſwelling of the veins, we muſt, without doubt, add the influence of the contraction of the right auricle: for, beſides the ſwelling of the jugular vein, which is produced by expiration, this vein hath another

other motion much quicker, which reſembles a palpitation, and which, if it is carefully obſerved, is always found at the ſame time with the firſt, and which continues after the Thorax is opened, and even after the power of reſpiration ceaſeth. For, it is not only at the diſtance of ſome lines, that the right auricle drives back the blood, and receives it again the moment following by alternate contractions and dilatations; but the effect of this motion extends itſelf even to the liver, under the heart, and above this organ, even to the neck, and in the Mammary veins; and alſo it appears in a dying animal, and when living in pain, as I have often obſerved, and which I am actually obſerving in a cat, now under view.

Laſtly, the alternate motion of the Diaphragm occaſions the like motion in the Vena Cava, which it drags and tightens as it deſcends, and in the following expiration it releaſeth it; whereupon the vein fills and becomes ſhortened. All theſe conſiderations muſt be again combined in expounding the motion of the brain and of the veins. Order now requires me to examine the motion of the venal blood, as I have that of the arterial.

For this purpoſe I have made uſe of ligatures, after the examples of Harvey and

Walleus *(m)*, who employed this method to prove, contrary to all antiquity, the return of the venal blood to the heart. Firſt I made a ligature on the Cava Inferior near the heart, or on the Superior, or on both at the ſame time. The effect in both caſes is always the ſame. The blood is collected between the ligatures and the extremities, I mean between the ligature and the limbs, the head, or the Abdomen; but the part of the vein that is intercepted between the auricle and the ligature, remains empty: ſometimes theſe ligatures prevent the motion of the heart, but that does not always happen. As for example, in a frog, I made ligatures on three of the principal veins; in a cat, on the two Cavæ and pulmonary veins, notwithſtanding which the heart continued its motions: yet I have obſerved at other times, in frogs, that the motion of the blood languiſhed extremely on making a ligature on the Cava Inferior (*n*). However, ſome error may happen in this obſervation; for I have ſeen quite the contrary in the right auricle, which hath drove back the blood into the part that was the neareſt to the Vena Cava, and

---

(m) In the Letters de Motu Sanguinis & Chyli, which are found in almoſt all the Editions of Bartholin's Anatomy, after the year 1641.

(*n*) See concerning this fact, a diſſertation which I have inſerted in the firſt volume of the Memoirs of the Royal Society of Gottingen, p. 273. and the following.

and propelled it even to the ligature and alſo into the jugular. This I have ſeen ſo often, that it is needleſs to enumerate the obſervations on animals on which they were made; and I have obſerved, ſince the year 1737, the appearance of a pulſe in the jugular vein of a cat, into which the right auricle had diſcharged ſome blood: even in frogs, this auricle fills very often the Vena Cava with blood, even to the liver, which I ſhall ſpeak of more at large hereafter.

When a ligature is made on the Vena Cava in the Abdomen, it ſwells under the ligature; but it does not always become empty between this ligature and the heart, becauſe the blood of the veins and of the liver enters again into that part of the vein, which is between the heart and the ligature. Yet this doth not prevent, in frogs, this portion of the vein from becoming empty. I have tied up the pulmonary veins in the ſame manner as the two Cavæ, and the effect was the ſame. The blood collected between the ligature and the lungs, ſwelled the veins of this Viſcus, and that part of the vein that was intercepted between the left ventricle; and the ligature became empty. Sometimes I made a ligature on the Vena Porta; at other times I only tied up the Meſenteric branch. The great Meſenteric vein manifeſtly ſwelled (although often very obſcurely) between the ligature and the inteſtines; but that part that was between the ligature and the liver was not diminiſhed, and had not leſs blood on being opened, becauſe it is in this intercepted part, that the ſpleinc vein, together with the gaſtric, and ſome others leſs conſiderable, diſcharge their blood.

I afterwards tied up a Meſenteric branch nearer the inteſtines. It ſwelled between the ligature and the inteſtines, but it did not at all diminiſh between the ligature and the liver; becauſe, in this ſpace, a great number of other branches diſcharge their blood, which prevents the perception of any diminution that may happen by any impediment on any ſingle branch. Riolan firſt, and others after him, oppoſed Harvey as to this fact *(o)*. On tying up the jugular vein, I always obſerved the ſpace between the head and the jugular to ſwell, when the other part remained empty, unleſs the force of expiration, and that of the right auricle, had diſturbed the experiment. On tying up the brachial vein of a dog, that part contained between the heart and the ligature became perfectly empty, and ſeemed as fine as a thread, which, on being cut, gave no blood; but the inferior part between the ligature and the paws ſwelled extremely, and gives much blood on opening, but without any pulſation.

(o) Prætermiſſ. the Paris edition, 1652. p. 165.

Laſtly, I tied up, in the ſame manner, the crurale vein of a dog, and that of a great rat; in both the part under the ligature ſwells, but the part above ſubſides.

Hence it appears, that always when there are no Anaſtomoſis's ſupplying blood above the ligature, or when the heart furniſheth none, the blood collects itſelf between the ligature and the head, the Abdomen or extremities; and that the part between the heart and the ligature becomes entirely empty. This is perfectly conſonant to Harvey's diſcoveries, experiments which, at this time, might be thought unneceſſarily repeated, had not Homobon Piſoni, an Italian phyſician, argued againſt the circulation; however, truths which are in a manner the foundation of all phyſic, cannot be too well eſtabliſhed. I have never obſerved, that the veins, when they are tied up, ſwell ſo as to become larger than arteries. As they are more flexile, and as the jugular vein, for example, may be dilated by an injection of mercury, ſo as to form a very large ſac; yet, on the other hand, the veins have a greater number of Anaſtomoſis's than the arteries, which ſerve to convey the blood into the veſſels that are free. Theſe anaſtomoſing veins may prodigiouſly dilate themſelves. I have in another place given a deſcription of a vein, which aroſe from a ſmall vein of one of the ureters, and which, after an obſtruction of the Vena Cava, ſerved to carry all the blood from the Iliacs to the Renal, and became as large as the Cava itſelf.

I have carefully obſerved the effects of a ligature on the ſmall veſſels: for this purpoſe I have thrice tied up a meſenteric vein in frogs; and, by the aſſiſtance of a microſcope, I have always found in theſe experiments, that much blood was collected under the ligature, at which place it loſt its motion and fluidity, without making any tumour in the vein; and the blood of the vein paſſed from the ligature, and returned towards the inteſtines, and went off by ſome collateral vein into larger veſſels, which brought it to the heart: but when any branch of a vein was entirely filled with fixed blood, in that caſe the blood of the trunk, without making any effort to dilate this trunk, abandoned the branch that was tied up, and paſſed into ſome branch near the ligature. Obſtructions of the veins equally aſſiſts us, as well as thoſe of the arteries, in accounting for an inflammation, attended with a tumour, redneſs, and pulſations, which are conſequences of the coagulations of the blood, as in a peripneumony.

I now come to ſpeak of what the microſcope diſcovers, with reſpect to the motion of the venal blood, which I have obſerved ſo often in fiſh and frogs (*p*).

And, firſt, as to its natural direction: it is as I have before alledged, from the extremities to the heart, ſo that it paſſeth from capillary veſſels, and of one globule diameter (it ſeems not conſiſtent to ſuppoſe ſmaller than thoſe) into thoſe of two or three globules diameter, and ſucceſſively into canals yet larger, and thus cometh to the heart. This I have often carefully, and with much pleaſure, obſerved on the Meſentery of a toad, wherein the Pexus's of the ſmall veſſels are more diſtinct than in a frog.

The motion of the blood in the veins is ſwift, and (*q*) is not ſo ſlow as the major part of phyſiologiſts ſuppoſe: 'tis alſo ſwift in the trunks, and in the ramifications of the veins of an animal in health; and it hath alſo a ſwift motion even in the capillary veins. This I have before related, and boldly again declare the ſame. I might rely on the celebrated M. Muſſchenbroeck,

---

(*p*) Malpighy made obſervations on the Meſentery of a Frog. This celebrated man is the firſt, who, ſince the year 1661, that is to ſay, before Lewenhoeck made obſervations on the circulation of the blood in the veſſels of the lungs: four years afterwards, in 1665, he obſerved the circulation in the Meſentery of a frog, and eſtabliſhed it by very fine obſervations.

(*q*) See Lewenhoeck, letter 102. and in the continuation of his Arcan. Natur. p. 131.

who ſaith, that the motion of the arterial blood is very ſwift at the place where theſe veſſels degenerate into veins (*r*). But 'tis beſt to uſe no other proofs than thoſe of experiments; in the greater part of thoſe which I have made with reſpect to the veins, I have obſerved that the velocity of their blood was not ſo great as that of the blood of the correſpondent arteries, but about three times ſlower; for the veins have generally their light or cavities about three times larger than the arteries. Nevertheleſs, it often happens, that no difference is at all perceived between the velocity of the arterial blood and that of the venal blood in the correſpondent veſſels. I have even ſometimes found a greater velocity in the vein; and 'tis obſerved often, that the veins preſerve their motion after the arteries have loſt theirs. In general, there is no occaſion of any calculation to prove that the velocity of the blood in both the Cavæ. is to the velocity of the blood of the Aorta, in an inverſe proportion to the ſize of theſe veſſels; and the ſizes and magnitudes of both the Cavæ are not quite three times larger than the Aorta, the proportion of 3578 to 1000, eſtabliſhed by Dr. Clifton Wintringham, much exceeds the truth, becauſe it makes the ratio of the diameter of the Superior Cava as 1865 to 1000, al-

---

(*r*) Eſſais Phyſique, p. 392.

though in man the Aorta is rather larger than the Cava Superior. Sometimes the ſmall veſſels and the capillary veins ſeem to bring back the blood with leſs velocity (*s*) than the trunks, and the velocities ſeem to be almoſt in a direct proportion to their magnitudes: and I have ſometimes found, that when two capillary veſſels re-united, ſo as to form but one trunk, that the velocity increaſed in this re-united branch. As in general, if the velocity of the blood in the Aorta becomes leſs in the capillary arteries, it muſt follow that the velocity the blood hath in the capillary veins muſt increaſe in the large ones (*t*): I have, nevertheleſs, ſometimes obſerved, that this velocity was greater in the capillary veins than in the trunks. This diminution of velocity in the ſmall veſſels, make the globules ſeem ſeparated and at a greater diſtance the one from the other; and this diſtance is pretty conſiderable: but, in the great veſſels, the globules ſeem to form a continued maſs; and this ſeparation of the globules produceth no diminution of their velocities. Theſe veſſels muſt certainly contain ſome other fluid that is inviſible, which forms the chain of connection between each red globule, without

---

(*s*) Malpighi, in his work before cited, p. 92.

(*t*) Butler of Blood-letting, p. 11. Robinſon Animal. Oeconom. prop. 10.

which it would be impoſſible to conceive how the force of the heart could propel the globules in a continual ſucceſſion; and, moreover, becauſe the veins are deſtitute of any contraction, and cannot aſſiſt the motion of theſe globules.

The venal blood, moving with leſs velocity, is more ſubject to loſe its motion than the arterial, and more eaſily becomes retrograde, which I ſhall hereafter more fully relate; yet we often obſerve it to preſerve its motion longer than the blood of the arteries, as I have before obſerved.

Theſe two motions differ; for the motion of the venal blood does not (as far at leaſt as I have been able to obſerve) undergo any accelaration either in the ſmall or large veſſels (*u*), but moves every where with an equal velocity; and it flows with the ſame equality of motion, when a vein is opened (*x*). I have nothing to do in this place with the reſpiratory pulſe, of which I have already ſpoken, nor with that ſpecies of pulſe which the retroceſſion of the blood of the auricle produceth in the Vena Cava, nor with that revived velocity which the venal blood acquires ſometimes after it had been entirely at reſt, or at leaſt after an accidental decreaſe of velocity.

(*u*) G. Adams, before cited, p. 45.

(*x*) J. A. Borelli de Motu Animalium, lib. 2. prop. 31.

It is obſervable in the veins, and even more diſtinctly than in the arteries, that the globules that move in the middle of the veſſels in the axis, have a ſomewhat greater velocity than thoſe which move nearer the parieties (*y*). I have not found that any angles of the veſſels diminiſhed the velocity of the circulation; but, on the contrary, I have obſerved, in places where the veſſels communicate, that the blood paſſed from the one to the other, under very acute angles, with as much rapidity as it did before.

The retroceſſion of the blood-globules againſt the parieties, at the diviſion of two veins, is made without any effort or reſilition; and, in this place, they paſs into the branch without changing their form, and continue their natural courſe: and theſe oſcillatory frictions of two columns, proceeding one againſt another, neither change their direction of a right line, nor the form of the globules.

No rotating or winding motion is obſerved in the veins; for all the venal blood is moved in right lines (*z*), parallel the one to the other. And, although the different appearances of the globules, which, in the capillary veſſels, ſeem ſometimes tranſparent,

---

(*y*) Malpighi Poſthum. p. 92.

(*z*) Malpighi, the work before cited, p. 92.

and ſometimes opake, and ſeem to ſhew a rotating motion, yet theſe different appearances depend much more on the flexions of theſe ſmall veſſels, than on any real change in the globules.

As Anaſtomoſis's are by far more frequently obſerved in the large veins than in the arteries; and, as there is found a large network of veins in the Meſentery of frogs, it affords an opportunity of confirming what I have ſaid concerning the ramifications of the veſſels: it is certain, the blood moves in them ſometimes in its natural direction, and ſometimes in a contrary direction (*a*). When two venal trunks communicate together by any neighbouring branch, ſometimes one of the trunks, and ſometimes the other furniſheth this communicating veſſel with blood; and this happens even in a healthy ſtate. I ſpeak not here of any preternatural oſcillations; and the following phænomenon is worth taking notice of, which is, That if a ſmall vein hath its inſertion in a venal trunk much larger, the force of the current of this large veſſel entirely ſtops the motion of the blood of the ſmall vein, although it is of ſome globules diameter, and hinders it from conveying its blood into the great trunk. This teacheth us, why nature hath ordered, that the veins of one globule diameter

---

(*a*) De Heide Obſerv. 85.

meter are never inſerted into trunks much larger than themſelves, and, why very ſmall branches re-unite, and form little trunks, which, afterwards re-uniting and forming a larger trunk, make a ſucceſſive gradation, thereby giving to the blood of the larger branches a force ſufficient to enter the trunks, notwithſtanding the reſiſting force of their currents. This ſhews the neceſſity of the thoraric duct, and ſtrengthens the opinion I have, that a ſmall lymphatic vein does not convey its blood into the Vena Cava; the lumbar vein, or the Azygos, as Pecquet, in his laſt experiments (*b*), Kulmus, and ſome others, have ſuppoſed.

The diſorders which the circulation of the venal blood undergoes are, as in the arteries, its perturbation, oſcillation, retrogradation, its want of motion, and, ſometimes, inanition, although this laſt is much leſs frequent than in the arteries.

The perturbation of motion in the venal blood conſiſts (as in the arteries) in this; its motion diminiſheth, and afterwards it recovers its velocity; ſometimes it ſtops entirely, and afterwards it recovers its motion with an increaſed velocity. Perhaps theſe irregularities and uncertainties in the velocity of the venal blood, made M. Queſnay (*c*) declare

---

(*b*) Nouvelle Inſertion du Canal Thorachique.

(*c*) Oeconomie Animal, firſt edition, p. 233.

That nothing certain could be eſtabliſhed on this article.

The oſcillation in the veins is a beautiful phænomenon, as well by reaſon of the magnitude of the venal network and Plexus's, but becauſe the venal blood, in all my experiments, appeared to be very ſubject to a retrograde motion, which neceſſarily produceth an oſcillation that remains as long as ſufficient ſtrength is preſerved to keep up the motion in the other veſſels.

The venal blood alternately moveth backward and forward in the ſame trunk, and ſometimes moves back to the inteſtines, before it returns to the heart. This is very frequently obſerved in fiſh and frogs; when theſe animals begin to grow languid. The blood ſeems to meet with ſome obſtacle and reſiſtance in the trunk, whereby it is forced back; and, as it goes back from the trunk, it is ſtopped by the force of the blood of the branch; but before it changeth its direction, ſome oppoſition is obſerved: for, the columns of blood, which follow oppoſite currents, run againſt and mutually repel each other. At other times, I have obſerved a trunk of the Meſentery to convey the blood even to the middle of the Meſentery, from whence it returned to the inteſtines by another trunk, whoſe blood had the ſame direction as that of the arteries.

On the other hand, I have found, in places where two tranſverſe branches were inſerted into the ſame trunk, that the blood balanced itſelf in the manner following: ſometimes, for a moment, it had a motion of retrogradation from the trunk into one or other of the branches, and preſently following its natural motion, it moved from two branches into the trunk; at other times, being repelled from the branch on the right ſide, it went retrograde through the trunk into the left branch, from whence it returned through the trunk into the branch on the right ſide; and this I have obſerved for above thirty minutes.

But the veins which communicate with each other, and in which the blood is ſeen to move in all poſſible directions, afford a more beautiful obſervation. The blood of a vein near the heart, and on the right ſide, paſſeth by a middle trunk into a trunk placed more to the left ſide, and the reſiſtance which it there finds, produceth an oſcillation; after this oſcillation, it deſcends by the left trunk towards the inteſtines, or elſe it takes again its former courſe by the middle trunk: ſometimes, in going out of this veſſel, it takes its natural courſe by moving towards the heart; at other times, it becomes retrograde under the inſertion of the veſſel of communication. Sometimes the motion beginning by a trunk on the right ſide near the inteſtines, the blood paſſeth firſt by an Anaſtomoſis into a trunk on the left ſide, from whence it takes its courſe, ſometimes to the heart, and ſometimes downwards towards the inteſtines (*d*); or elſe it continues its motion in its trunk towards the heart, according to the common courſe of circulation: alſo, in the ſame venal trunk, 'tis obſerved, that one part of the blood carried thereto from a ramification, moves towards the heart, and another part at the ſame time towards the inteſtines. What I have ſaid with reſpect to the trunk of the right ſide, ſoon after happens in the left trunk: ſometimes the blood returns thereto, and ſometimes it paſſeth by the middle canal, from whence it is either brought to the heart, or repelled towards the inteſtines. The difference of the angles, which the middle branch makes with the two trunks, creates no difference in the velocity of the blood in its paſſing from theſe two veſſels; and it paſſeth from the left trunk under an acute angle, with as great a velocity as it had at its firſt paſſing from the trunk on the right ſide under an obtuſe angle. I have diſtinctly obſerved this kind of conflict taking place between the different columns, which are carried away by their motion; ſo that this direct and indirect motion

---

(*d*) De Heide hath perceived ſomewhat like this.

tion of the blood produceth a ſort of confuſion in the veſſels (*e*). And I have very often found, as I have ſaid elſewhere, that of all the veſſels, there are none in which the motion is longer continued than in the branches of communication.

Moreover, I have ſeen in one and the ſame venal trunk, and three of its branches, the following different motions. In the trunk there was an oſcillation: one moment the blood moved retrograde; the next moment, as it moved from the part the neareſt to the inteſtines, it was carried in its natural direction towards the heart, or at leaſt into ſome branch conveying the blood that way. The ſuperior branch contained but few globules, but they had a very remarkable oſcillatory motion, ſo that they were alternately carried towards the inteſtines, and the next moment towards the heart. With reſpect to the middle branch, the blood of the trunk ſometimes entered it, and, at other times, it was repelled from thence very ſtrongly. Laſtly, after the blood had deſcended with a great velocity into the inferior trunk, it afterwards relinquiſhed the ſame, and aſcended againſt its own weight. All this was obſerved after I had divided two of the principal trunks of the Aorta.

(*e*) This is the motion that ſo much ſurprized Lewenhoeck, Contin. Arcan. Natur. p. 116.

Sometimes an oſcillation ariſeth in the following manner. The blood ceaſeth entirely its motion, and recovers it again in a retrograde direction, and it paſſeth again into ſome other branch, by which it returns to the heart (*f*). The direct and natural motion ſometimes ſucceeds an oſcillation, and at other times a retrogradation is followed by a total ceſſation of motion, which generally happens, as I ſhall relate elſewhere, when the heart is taken away. It ſeems that it is to this oſcillatory motion, that the celebrated M. Whytt attributes the motion of the blood in the ſmall veſſels (*g*); but this opinion will be deſtroyed, if we conſider that this motion never takes place in an animal in health; and that it cannot be reckoned among the natural cauſes of the motion of the blood, but is preternatural, and a diſeaſe.

Nothing is more frequent than the retrograde motion of the venal blood, and often when a frog is obſerved with a microſcope, and the arteries are found to keep up very regularly their pulſations, yet the venal blood begins to turn back, and return to the inteſtines; they having no valves to oppoſe it, I am apt to think that a wound is often the cauſe of this retrogradation, becauſe the reflux

---

(*f*) Malpighi in the ſame place, p. 92.
(*g*) On Vital Motion, p. 96.

flux of the blood is very common when any venous veſſel happens to break; I own it ſometimes takes place, although no veſſel is open; ſometimes a coalition of the blood occaſions it (*h*): and this is evident, becauſe the natural motion again takes place after the diſſolution of the coagulum that was formed by the red globules. At other times I have ſeen the blood repelled by the grumous blood of a vein that was divided: I have alſo ſeen the blood return from a large trunk into a ſmall branch of ſome globules diameter. This is what a friend of the celebrated M. Baker took for the ſecretion of ſome extreme fine humour (*i*).

The oſcillation, and the motion of retrogradation, are almoſt always followed by a ceſſation of motion; but this is not of any continuance, provided the vital ſtrength is not impaired: and, in this caſe the blood, which was before at reſt, is obſerved to put itſelf again into motion, in order to purſue its natural courſe. In animals that have warm blood, the motion of the blood ceaſeth much ſooner; and the veins of the Abdomen, that is opened in a living animal, become varicous and ſwelled by the collection of the blood that had loſt its motion, as the

---

(*h*) As in the example mentioned by Lewenhoeck, Experim. & Contemp. p. 208.

(*i*) See this author's work, before cited, p. 136.

great Boerhaave demonſtrated to his auditors. This ceſſation of motion begins in the ſmall veſſels; in fiſh it begins in thoſe that are the neareſt the tail; in frogs, in the network and ramifications of veins of one globule diameter, although the blood in the great trunks at the ſame time continueth its motion (*k*). Sometimes the motion ceaſeth in ſome branches, and continues in others; and indeed it hath happened, that ſmall veins, of one or two globules diameter, preſerve their motion, after it hath entirely ceaſed, not only in the trunks of the veins, but alſo in the larger arteries. I have obſerved in a Toad, that the ſmall veſſels preſerved their motion, although I had taken away the heart; but this ſeldom happens. When the blood is at reſt, the veins are often very full, and this blood, by reaſon of its Striæ, reſembles oil; and the day after death, I have found the veins full of blood, that was dry and immoveable: yet, ſometimes the veins appear empty at the beginning of the experiment, by reaſon of the animal's faſting, or of ſome other cauſe. In other ſubjects, they are only empty in ſome particular part; and in others, they contain blood in their whole extent, but in a ſmall quantity and of a yellow colour. Laſtly, after apparent death, the veins are generally

---

(*k*) See De Heide Exper. 8.

either empty, or at moſt but one half full, eſpecially if the animal had loſt a little blood: alſo, 'tis not ſeldom we find in animals that have warm blood, and even in man, veins either empty or filled with Bullæ of air, either in the brain or other parts. Almoſt all authors that have treated on this ſubject, make the veins full of blood after death; which is true with reſpect to the Vena Cava, and the other large veſſels, into which the blood is forced by cold or other cauſes (of which I ſhall ſpeak in the ſixth chapter); and, by this means, there is a congeſtion of blood at the entrance into the lungs, but this is not conſtant in the ſmaller veins.

CHAP. V.

The Effects of Bleeding in the Courſe and Direction of the Blood.

I Employed myſelf in anatomy at Paris, when M. Silva, who was in conſiderable repute among the practitioners there, publiſhed his treatiſe on bleeding; a work that met with a multitude of opponents. His ſyſtem was founded on this principle of Bellini, who ſaid, That when a vein is opened, the blood of that vein, as alſo that of the neighbouring veins, and alſo that of the artery that furniſheth them with blood, acquires a new degree of velocity, and projects itſelf towards the orifice. By this means a great derivation of the humours is made on the part where the vein is opened; and the other veins, which have no communication therewith, undergo by this means a conſiderable revulſion.

Many authors (*a*) oppoſed this doctrine, but eſpecially M. Queſnay (*b*); and with him a perſon of a ſuperior genius, the celebrated M. Senac, who, in his letters publiſhed under the fictitious name of Julian Moriſſon (*c*), and afterwards in his medical eſſays on the anatomy of M. Heiſter, and, laſtly, in his treatiſe on the heart, hath, with much honour, oppoſed the Bellinian doctrine. As to the practiſing phyſicians, ſome have followed the ancient doctrine, and ſome are friends to the new one. Since this I have been deſirous of ſatisfying myſelf by

---

(*a*) M. Chevalier Obſervations Critiques ſur le Traitè des Saignees, 12. 1730. M. Roger Butler Eſſay on Blood-letting, 8. London 1734. Brown Langriſh modern Theory and Practice of Phyſick, 8. London 1738. M. Martin de la Phlebotomie & de l'Arteriotomie, 12. Paris 1741. M. Roland Jackſon de Vera Phlebotomiæ Theoria, 8. London 1747. Giles Watts of Revulſion and Derivation, 8. London 1754.

(*b*) Obſervations ſur la Saignee, 12. Paris 1730.

(*c*) Lettres ſur le Choix des Saignees, 12. Paris 1730.

experiments, what real changes bleeding could produce with reſpect to the courſe of the blood; nothing ſeemed more proper to decide this controverſy, than to bleed a living animal, that we may be able to diſtinguiſh the blood through the membranes of the veins, and obſerve the changes it at this time undergoes. For this reaſon, I was determined to take the method which M. de Heide had began, and to add new experiments to thoſe he hath left us, and which have their uſe. I have made uſe of a great number of animals in theſe experiments; ſome whereof were made with M. Remus, and the major part of them I made in the ſummer-time of the year 1754. Bleeding, by opening the veins, is not attended with much difficulty in a man of a little experience; for the meſenteric veins of frogs, and of toads eſpecially, are very viſible, and are eaſily opened with a lancet: but bleeding by opening of the arteries, which are ſmaller, not ſo high coloured, and harder, is not quite ſo eaſily performed; yet I have opened a great many, and I do not at all regret the time I have employed in making theſe experiments.

The firſt thing I was to examine was, whether the velocity of the blood increaſed in the vein that is opened, and in its adjacent ones. This ſuppoſition being the foundation

of

of all the Bellinian theory (*d*), if it is not true, the whole ſyſtem is deſtroyed. The celebrated author, under the name of Julian Moriſſon (*e*), abſolutely denies that the blood moves with a greater velocity from the vein that is opened, than from thoſe that are intire; or that bleeding produceth any acceleration in the motion of the venal blood. The late M. Hamberger maintains in a manner the ſame ſentiments; for he reckons the augmentation of the velocity, produced by bleeding only, as 200 to 201 (*f*). As for my part, I have very often obſerved, and as often as I deſired, the reſult having been always the ſame: I ſay I have obſerved, that whatever the direction of the blood was, in the vein that I opened, whether it moved in its natural courſe to the heart, or whether by any retrograde motion it was carried towards the inteſtines, or whether it was upon the poiſe or at reſt; or, laſtly, whether the heart was taken away, or the Aorta's tied up or divided. In all theſe caſes, the blood iſſued from the vein that was opened with a velocity much greater than that which

---

(*d*) Propoſit. 1.

(*e*) See the fifth letter; alſo a theſis read at Paris, in 1734, on this ſubject, when Baron was preſident; and Les Eſſais de Phyſique, edit. of 1735. p. 522. 524.

(*f*) Diſſertatio de Ven. Sect. n. 43.

it had in the intire vein (*g*), and even with a greater than it hath in the arteries. At firſt it iſſues from the vein in winding motions, as if it was propelled by the weight of a large column of water, ſufficient to force and ſtrain the tube; and its velocity, which is the greater at the wound, diminiſheth, in proportion as the blood extravaſates into the Laminæ of the Meſentery; and the globules, which were at firſt collected together, ſo as to form but a very ſmall paſſage, by degrees ſeparate themſelves, and the channel of the current becomes conſiderably larger. Two oppoſite currents, both ſwift in motion, haſten to the orifice of the vein, yet there comes more blood, and with a greater velocity from the heart; and the column that comes from thence, repelling that which comes from the extremities, furniſheth almoſt all the blood which is loſt by bleeding.

Afterwards I obſerved that the opening of a vein produceth a very ſwift motion of the venal blood, even after it hath been a long time at reſt (*h*), and the heart taken away; and that this velocity takes place not only in the vein that is opened, and the branches communicating therewith, but alſo

---

(*g*) Theſe experiments are conformable to thoſe of De Heide. See his work, p. 2. 4.

(*h*) See De Heide, p. 4. 8. and Hale's Hæmaſtatics, p. 165.

in the adjacent trunks that communicate with it, and even in the ſmall capillary veins. This experiment I always make with ſucceſs, unleſs the vein that is opened hath loſt all its blood, or is become dry: it alſo ſucceeds, as I have before declared, after the heart is taken away; and after a ligature is made on the two great trunks of the Aorta, from which the whole body of this animal receives its blood. Laſtly, bleeding ſeems to have ſo great a power to vary the courſe of the blood, that it produceth a motion contrary to the common laws of circulation (*i*): it makes the blood move retrograde from the heart towards the inciſion, and it accelerates the motion of that which was coming from the inteſtines; this produceth in the orifice an influx of blood from two oppoſite columns. 'Tis entertaining to obſerve this oppoſition, ariſing between theſe two columns; and this is ſometimes the more diſtinct, as they are of different colours: for the current from the heart is of a much deeper red than that which comes from the inteſtines, which is more pale.

This rapid motion conſiderably, and almoſt entirely emptieth the adjacent veins; as the blood which returneth from the heart, hath a greater velocity than that which

---

(*i*) De Heide, p. 5. 8.

cometh from the inteſtines, its motion is continued longer; and this difference of velocity and duration is nearly in the ſame proportion: ſometimes the current from the heart only ſupplieth the blood.

The adjacent veins empty themſelves towards the inciſion, not only by a direct, but alſo by a retrograde motion (*k*). And, if their blood ceaſeth its motion, bleeding reſtores it; and, at the ſame time, gives a fluidity to the congeſtion of globules, which this want of motion had cauſed to degenerate into a kind of oily maſs.

After having made two orifices in one vein, between which the blood was at reſt, and at neither of which orifices the blood diſcharged itſelf, I obſerved at laſt it determined itſelf towards the inferior orifice, by which it went out.

There happens but little alteration, either with reſpect to the velocity, or with reſpect to the evacuation in the veins of the Meſenſentery that are the moſt diſtant from thoſe which are opened, as hath been juſtly obſerved by the author of thoſe letters under the name of Moriſſoh (*l*). The congeſtions of the re-united globules recover their motion by bleeding, and the grumes which ſometimes diſcharge themſelves at the orifice

---

(*k*) De Heide, p. 4.

(*l*) Lettre huit Eſſais de Phyſique, p. 526. 529.

like red clouds, contribute to re-eſtabliſh a free circulation. Laſtly, and what is not commonly obſerved after ſeven times bleeding, I have ſeen the motion of the venal blood to continue for two hours, even in the ſmall capillary veſſels. By all this it appears, how neceſſary bleeding is to re-eſtabliſh any ſuſpended circulation in perſons drowned, and in ſoporiferous diſtempers.

Alſo it appears, that bleeding brings the blood from the adjacent part into that where this operation is performed, and that therefore there is nothing in the doctrine of derivation that is contradictory to experiments, provided its effect is not hindered by the valves. Thus bleeding in the jugular emptieth the heart, the right auricle, and the lungs, becauſe there are no valves oppoſing: this I am convinced of by other experiments on a dog, in which I ſaw diſtinctly, that the blood found no obſtacle from the right auricle into the external jugular. It is yet more certain, and more generally known, that bleeding in this vein eaſeth the brain.

We ſee alſo that the coagulums produced, as great men have obſerved, by fear, cold, fevers, or other cauſes, may be diſſolved and removed by bleeding, and that it gives a fluidity to the blood that was at reſt.

It is not only in any particular part, but even in all the animal body, that the coagulated and viſcid blood may be reſolved by this increaſe of velocity, which bleeding produceth, as any one may find by experiments on frogs.

Another theorem may be drawn from theſe facts, which is, that, as all the veins of the body are connected to one another, the neareſt to thoſe which are opened, empty themſelves the moſt, and thoſe which are more diſtant, diſcharge themſelves into the former; their blood acquires a velocity ſomewhat greater: but theſe effects always diminiſh in proportion to the diſtance from the vein that is opened, and at length abſolutely ceaſe (*m*). For, as every hæmorrhage ſtops ſoon enough without aſſiſtance, and in artificial bleedings the blood very ſoon ſtops, even without any bandage, therefore it never happens that all the veins entirely empty themſelves; and thoſe which are diſtant from the orifice, empty themſelves but very little, as I have related in giving the reſult of the experiment. Although derivation, and even a ſmall revulſion in the veins, are proved by theſe facts, yet they are not ſufficient to prove

---

(*m*) A man of a great genius, M. G. C. Oeder profeſſor at Copenhagen, hath cleared up this matter in his Inaugural Theſis, publiſhed at Gottingen, ann. 1749.

that theſe effects are produced in the utmoſt extremities of the body.

But the ſecond, and moſt important part of the queſtion is, to know whether bleeding accelerates, alſo the motion of the arterial blood as laid down in the Bellinian doctrine (*n*). What proves in effect the importance of this part of the queſtion is, that 'tis not on the derivation, or the revulſion of the veins, that we depend in practice, but on that which ought to be made by the arteries correſponding with the vein that is opened. For inflammatory diſtempers, as phrenſies and the pleuriſy, are conſidered as diſeaſes of the arteries; and, laſtly, the great Boerhaave, and with him almoſt all practitioners, expect by opening a vein, to determine that way the courſe of the blood, which ſwells the inflamed part, and to remove obſtructions from the extremities of the arteries, and return a fluidity to the blood, it is very difficult to decide this queſtion by experiments; their reſult hath not been always the ſame, and that which I have made on myſelf, did not anſwer my expectation. For in a continual fever, many of which, and very troubleſome ones, I laboured under at Gottingen; I was blooded: before the

---

(*n*) See Bellini, propoſ. 1. and H. Schulze, in a diſſertation, intitled, Præjudic circa Ven. Sect. Opinion. &c.

operation, my pulſe had 122 pulſations in a minute; it retained the ſame velocity during the operation, and after it was finiſhed. As to making experiments on living animals, there is another difficulty attends it, which is, that the motion of the arteries decreaſeth more ſlowly than that of the veins, and we muſt wait many hours before this diminution becomes ſenſible; and, unleſs you wait for this diminution of motion, the motion of the arterial blood, which is of itſelf ſwift, becomes not accelerated by bleeding; and, if you defer the operation, until the motion of the arterial blood is ſlackened, often, in this interval, the veins become dry and empty, and are no more proper for the experiment.

I intend honeſtly to report the reſults of ſixty-two experiments. In thirty-ſix, I have not conſidered the arterial blood; or, at leaſt, its velocity was too great to be augmented by bleeding. In twenty-ſix others, I carefully obſerved the change produced in them by this operation. In five bleedings I could not find any alteration was produced thereby, with reſpect to the motion of the blood in the correſponding artery; and with reſpect to the adjacent arteries, there was only one of them in which, either by the effect of bleeding or of ſome other cauſe, the blood of the artery moved a little ſlower after the operation.

In twenty-one other caſes, bleeding increaſed the motion in the arteries, either if it was only diminiſhed or totally ſuſpended: ſometimes it began again its motion by balancing itſelf; at other times it ſucceſſively recovered its natural motion an hour after it had been at reſt, both the arterial and venal blood moved pretty ſwiftly, and this motion was increaſed by bleeding even in the arteries themſelves.

Eight of theſe experiments were made on animals which had but little or no blood in the adjacent arteries; and when the blood began to run out of the orifice of the vein, a greater quantity of blood entered the artery, and its velocity increaſing with the quantity, it preſently became full; and this happened after I had taken away the heart.

I have obſerved that the blood which was at reſt in an Aneuriſme, produced by Arteriotomy, and alſo that the blood in the adjacent artery was put into motion by bleeding, and flowed, though in a ſmall ſtream, through the Aneuriſme and the trunk; it afterwards ſtopped its motion, and ſome globules were obſerved to have a retrograde motion in the artery. A ſecond bleeding in the correſpondent vein procured again a direct motion in the artery, and the trunk, from which it took its riſe. When a vein is opened after death, or after the heart is taken away, it produceth ſome motion in the artery.

By what I have related, it appears that the caſes wherein bleeding hath increaſed the motion of the artery, are more numerous than thoſe wherein it hath not; therefore no regard is to be had to the objection I am now endeavouring to prevent. Theſe increaſed motions, you may ſay, have not their ſole cauſe from bleeding; but they have too frequently happened after it, to attribute them to any other accidental cauſe. Why, in effect, ſhould this happen the very moment of bleeding? Therefore we may conclude with reſpect to the ſecond queſtion, without any doubt, that after bleeding, the motion of the blood is accelerated in the correſpondent arteries bordering on the vein that is opened. And my experiments confirm the Bellinian doctrine on derivation, which prove, that when a vein is opened, the blood ſupplies more abundantly the arteries of the part where the vein hath been opened: the objection which may be made with reſpect to the valves, in the prior experiments on the veins, have no force againſt theſe laſt.

What fully eſtabliſheth the revulſion of the artery, is, that the blood collecting in the place where the vein is opened, and the quantity of the whole maſs not increaſing at this time, the arteries bordering on thoſe which are diſcharged by the orifice, empty their blood into theſe laſt, finding therein the leaſt reſiſtance; and this diminution of reſiſtance taking place in the contiguous veſſels, the effect muſt be extended to arteries at ſome diſtance; yet it doth not follow, that it ſhould extend to thoſe at a much greater diſtance, becauſe the ſmall Anaſtomoſis's between two, are not ſufficient to create a very ſwift motion of the blood of one artery to another that is very diſtant; ſo it cannot be expected that the blood of the brain can throw itſelf on the crural artery through the ſmall Anaſtomoſis's of the ſpinal marrow, or of the veſſels of the baſon (*o*).

And if, moreover, we add to this, that a man becomes weak by loſing blood, and that the force of the heart being diminiſhed, whereby the reſiſtance which is made by the arteries in the parts oppoſite to that wherein bleeding is performed, is found proportionably greater than in a man in health. It is a juſt doctrine to determine a greater quantity of blood to the parts whoſe vein is opened, rather than to the brain or other parts, wherein nothing hath diminiſhed the reſiſtance of the veſſels. We ſee then, that derivation and

---

(*o*) It will appear in the Faſcicul. Iconum 4. plate 4, and 7. That the arteries of the Coccyx and Sacrum are united with the arteries of the ſpinal marrow by frequent Anaſtomoſis's.

revulſion are proved by theſe experiments; for there ariſeth a true revulſion from the head, always when leſs blood is carried thither from the heart, and thereby the veſſels of the brain are conſequently leſs ſwelled.

I confeſs that all this is true only during the time of bleeding; but when the orifice is cloſed, all, as I ſhall declare preſently (*p*), enter again into their prior ſtate and uniformity (*q*): but the motion of the blood almoſt always loſeth ſomewhat of its force and velocity, which is another great intention, and even what commonly phyſicians aim at, when they direct bleeding in fevers. For, the grand point is, to reduce the exceſſive force of the heart, which hardens the coagulum, and fills the veſſels already too much dilated, and occaſions dangerous extravaſations from the obſtructed veſſels into the cellular membrane; and alſo increaſeth, by a great heat, a tendency in the blood to a volatile putreſcency. It remains for me now to conſider the other phænomena which happen when a vein is opened.

I made two inciſions in the ſame vein; the ſuperior, which was the neareſt the heart, retarded the flowing of the blood by

---

(*p*) See De Heide, p. 8.

(*q*) Bellini ſuppoſeth, propoſ. 11. That the velocity is greater after bleeding than before; but this is not conſonant to experiments.

the inferior inciſion, but it did not ſuppreſs it. You find by this experiment, what may be expected by bleeding, in order to ſtop a hæmorrhage. It acts chiefly by weakening the action of the heart, whoſe force we endeavour to diminiſh, untill the hæmorrhage ſtops naturally, or until remedies may be applied for that purpoſe.

The ſtream of blood iſſuing from the vein, by degrees ſlackens; and the globules, which with force ſeparated themſelves in different directions, collect themſelves round the wound; ſo that it becomes ſurrounded with a large ſpot, the colour whereof is always the leſs red in the places the more diſtant from the orifice of the vein, and more red in thoſe that are nearer thereto. We obſerve alſo in the wound the grume, whereof I have ſpoken in the ſecond chapter. This grume is formed by the re-union of the red globules, which I have obſerved in a vein that I opened by the ſide of the Meſentery that was the neareſt to the glaſs, and which interſects the middle of a tubercle, that is white and membranous, fixed to the vein.

After the orifice of the vein is cloſed, the blood re-aſſumes its former motion, ſometimes it moves in a direction from the heart, when the grume is taken away by friction, the wound opens again, and the hæmorrhage returns (*r*). Sometimes the current re-eſtabliſheth itſelf in the vein, ſo as to fill the veſſel; at other times it begins by two or three ſmall diſtinct torrents, carried through the white cloud. If one half of the vein is divided by inciſion, this orifice remains open; and yet, what is very ſurpriſing, the circulation is continued above this orifice, by the ſmall portion of the canal which is preſerved.

If the vein is entirely tore, ſometimes no blood iſſues from it; but, in this caſe, the vein either cloſeth itſelf at its extremity, like a round tuberoſity full of viſcid blood, or elſe it forms a kind of cone cloſed at the Apex, in which the blood becomes fixed. I have at this time obſerved the coagulated blood to repel that which came from the heart, and which went to the extremity of the vein and made an oſcillation with that blood; but, ſometimes when it is a conſiderable vein which hath been thus divided, it gives blood abundantly. When a vein is intirely cut, ſometimes it gives blood from the ſeat of the heart; at other times it gives none, eſpe-

---

(*r*) See De Heide, p. 3. By this we ſee how neceſſary reſt is in wounds of the veſſels, and what care ought to be taken not to cleanſe them too roughly, for fear of deſtroying that favourable grume that cloſeth the mouth of the artery.

cially if the animal is a little feeble, it forms at its extremities a ſack full of blood, which begins to balance itſelf by moving backwards and forwards with that which moves retrograde from the heart.

The opening of an artery produceth nearly the ſame phænomena with thoſe of a vein; but the motion of the arterial blood being much more ſwift than that of the venal, it iſſues from the wound with a velocity much greater: it is ſurpriſing; and this velocity is greater than it was before the inciſion.

The opening of an artery equally accelerates the motion of the blood, as well in the artery that is opened, as in the correſpondent ones; and this acceleration extends to a conſiderable diſtance. From the inciſion, we obſerve to iſſue not only the blood which comes from the ſeat of the heart, but alſo a column, which, by a retrograde (*s*) motion, returns from the inteſtines, and which makes itſelf a paſſage, notwithſtanding the reſiſtance it meets with from the column which comes from the heart.

When either the heart or the Aorta is cut off, generally the blood of the meſenteric artery moves retrograde; this artery emptieth itſelf, and a perfect reſt enſues: yet I have obſerved, in opening this meſenteric artery, that the ſmall number of red globules, which

---

(*s*) See De Heide, p. 3. 6.

were diſtributed in all its extent, and which filled but a ſmall part of it, continued to flow.

When the blood ceaſeth to flow from the wound, for this happens eſpecially if the animal is weak, and a ſecond inciſion is made in the artery higher than the firſt, it revives the motion of the blood, and eſtabliſheth it, as well between the two inciſions as under the firſt (*t*).

After the current of blood flows from the wound in the artery, the motion of the blood by degrees abates, and is diſcompoſed; and we obſerve globules, either ſlowly to move one by one, or a new wave coming intire from the heart: but the column which comes from the inteſtines the firſt, becomes weak, and, after ſome conteſt, the force of the column from the ſeat of the heart prevailing, at laſt repels the other, and compelling it to retake its courſe under the wound, it puts again the motion of the blood into its natural ſtate.

The wound of an artery cloſeth itſelf in the ſame manner as that of a vein: there becomes formed under the inciſion a kind of ſmall ſpot, like a cloud, which at firſt is red, afterwards it changeth, and becomes pale about its edges. In the middle a grume is formed by the re-union of ſome globules, the motion of the blood by degrees abates,

---

(*t*) De Heide hath obſerved ſomething like this, p. 7.

even in the artery, until this fluid, having paſſed beyond the grume, takes up again its former courſe.

It is very certain, that it is a coagulated fluid which cloſeth the wound of the artery. I have obſerved red globules to force a paſſage through this cloud, and flow out of the wound into the Laminæ of the Meſentery, and the hæmorrhage returned by rubbing the artery, and taking away the gluten which cloſeth the wound. When the inciſion is large, it preſerves its diameter without diminution, and it doth not become ſmaller by any contraction: after the wound is healed, the blood takes again its natural motion (*u*), or rather a motion ſomewhat more ſlow; often all the blood of the arteries flows out of the inciſion, and they remain entirely empty (*x*).

It is not uncommon to find an artery that hath been opened, more dilated in the place of the Cicatrix, and an Aneuriſme formed there. I cut an Aneuriſme of this kind, but nothing flowed from it; and I obſerved a membrane, ſurrounding it circularly with a ſmall opening, obſtructed by a ſmall grume of blood. I ſaw this blind ſack to fill by degrees with blood, and which came from a column from the heart, as being the ſtrongeſt;

as

(*u*) See De Heide, p. 8. (*x*) Ibid. p. 3. 6.

as ſoon as it was filled, the blood paſſed beyond and entered an adjacent branch.

There is alſo another way whereby wounds of the arteries cloſe themſelves, and that is by the contraction of their membrane, not that it is muſcular; for nothing of this appears in the arteries of frogs, but by a natural contraction which brings the fibres towards the Axis, and draws them from the contact of the reſt of the membrane, and which takes place even in the arteries of a dead ſubject.

An artery that is tore and not cut, contracts itſelf and forms a round tumour, from which I have ſometimes obſerved there did not iſſue one drop of blood, although it was ſupplied with it from a neighbouring branch; at other times it gave blood. This makes me believe, that the umbilical arteries of a fœtus that is ſtrong, would bleed if they were cut; unleſs a ligature is uſed, and that thoſe of a fœtus that was weak would not. Therefore the experiments of the late M. Schuize, who ſuppoſed that without any danger the binding and tying up the chord might be omitted, and thoſe of his adverſaries who have ſeen bad conſequences by this neglect, may be equally true. I have obſerved on the fœtus of a cat that had been four weeks pregnant, which fœtus I took from the cat by the ceſarean operation, that the umbilical arteries always beat when the heart of this little animal contracted itſelf (*y*).

When the artery and the vein are opened at the ſame time, the blood flows equally from both, but there is no alteration in its velocity. The blood of the meſenterick artery of a ſheep ſpurted ſix feet high, I opened another artery, at which time the ſpring of neither exceeded one foot.

Moreover I have obſerved, that on opening the great veſſels of dogs or ſheep, the firſt ſpring of the blood only extends itſelf to a certain height, which diminiſheth conſiderably after the firſt; and it is only with reſpect to this firſt ſpring of the blood, on which the calculations are founded, whereby we endeavour to conſider the motion of the heart.

CHAP. VI.

Of the Cauſes of the Motion of the Blood.

THERE is yet remaining the moſt difficult part of the work, which is, to diſcover the cauſes producing all theſe different motions which I have hitherto deſcribed. The

---

(*y*) M. Smellie, an excellent midwife, hath obſerved in infants where the Funis hath not been ſufficiently bound, that a very conſiderable quantity of blood was loſt. In his caſes and obſervations.

The heart is generally allowed to be the firſt; to which is added the contraction of the arteries, the compreſſion of the muſcles, the action of the nerves; and ſome authors beſides add thereto the ſuction of the capillary veſſels. Beſides the effects of theſe different powers, I will enquire into the effects of weight, of cold and heat, of the internal air, and laſtly into the effects of that unknown power, for which I ſhall preſerve the name of attraction, until new experiments ſhall better diſcover to us its nature; for all theſe cauſes act independent of the heart, and produce conſiderable motions in the blood. The motion of the heart is certainly the principal agent of the motion of the blood, in animals that have cold blood: this fluid, I confeſs, preſerves its motion for 40 minutes after the heart is taken away, and even longer, by the power of the cauſe I have laſt mentioned; but the order and conſtancy of theſe motions, even in animals of this kind, ceaſe immediately by deſtroying the heart, whoſe want of motion produceth the ſame effect in all the other parts of the animal body, and whoſe motion re eſtabliſheth all the others; all the humours return unto and go from the heart. In animals that have warm blood, its motion hardly ſubſiſts one minute after the heart is deſtroyed. A motion of this conſequence, deſerves a very careful examination, even in the different ſtates of the blood's motion, I will, in this inquiry, only conſult experiments (*a*): and I ſhall not mention the opinions of different authors, who have treated on this ſubject, which would extend this work to an exceſſive length.

It may be ſaid, that the motion of the heart begins at one of the Venæ Cavæ: for, when we tie up or cut theſe veins, the motion of the right auricle, and that of the right ventricle, ſoon abates, and preſently intirely ceaſeth; theſe veins are by nature the firſt agent of the heart's motion.

There is a conſiderable part of the Cava which hath a power of contraction, ſomewhat like a muſcular power. In frogs, and animals that have cold blood, that part of the Cava which goeth out of the liver, hath (*b*) a pulſation, as alſo its branches, from this going off, even to the heart; the Superior Cava, which extends beyond the lungs to the head and neighbouring parts, and even to both the brachial veins, have a motion of contraction very ſenſible, by the aſſiſtance of which they convey the blood into the auricle; the contraction of the Vena Cava takes place alſo

---

(*a*) Theſe experiments are to be found in their order in the 17th ſection of the ſecond diſſertation on Irritability.

(*b*) It giveth two branches to this viſcus, and the third deſcends the length of the lower belly.

ſometimes under the liver, eſpecially if the Aorta is tied; according to nature the motion of the Vena Cava always preceeds the contraction of the auricle. In a dog, and in animals like man, the Vena Cava, and eſpecially its ſuperior trunk, hath a power of contraction, as there is alſo, though leſs ſenſible, in the Inferior Cava, even to the Diaphragm and liver; for I ſpeak not here on that motion, which depends on reſpiration. I have obſerved in a dog, this motion of the Vena Cava to remain full five hours after death. It is therefore certain that the motion of the heart begins by the contraction of the Vena Cava, which drives the blood into the right auricle by true pulſations. Old experiments made me ſuſpect this; and I am convinced thereof by repeating them. As the right ear is larger than both the Cavæ, it makes no reſiſtance to the blood, which it receives from them, it ſwells by degrees, and, when its diſtenſion is increaſed to a certain degree, the irritation which it occaſions, pronuceth a motion in the auricle. I have often ſeen the motion of the auricle to begin at the blind ſack which lieth on the Aorta, and deſcend towards the inferior part. I have alſo obſerved the part which is on the right ſide in man, to draw near that part which is on the left, and reciprocally the left alſo to be brought towards the right; and the convexity, which is between the right and left extremities of the auricle, to lower itſelf at every pulſation. Its conſtriction is compoſed of all theſe beforementioned motions.

In a dead ſubject, by preſſing the auricle, we make the blood re-paſs partly into the ventricle, and partly into one of the Cavæ, there being no valves between theſe veins and the right auricle, and the valves which are at the entrance of the jugular vein being apparently too weak to reſiſt the effort of the blood.

Without doubt, in the common order of the circulation, both the Cavæ reſiſt the blood of the auricle by the preſſure of that which they themſelves contain, by reaſon of which reſiſtance it is diſcharged into the right ventricle. For the jugular vein doth not appear to have any pulſation in a man in health, or in any animal at reſt: but when a man exerts his ſtrength, or whoſe blood by any cauſe whatever paſſeth with difficulty into the lungs, it appears by the ſwelling of the neck or face, that ſome blood re-paſſeth from the auricle into the jugulars, and apparently into the Venæ Cavæ; perhaps this is generally the reaſon of the largeneſs of the neck, which many women have after delivery, and which is too frequent in my country. An ancient preſage of the Romans ſeems founded upon this obſervation. 'Tis true that they extended too far their opinion concerning this enlarged ſize of the neck. I have very diſtinctly obſerved the blood to re-paſs into the Mammaries, the ſubclavians, and other veins: and this I am now obſerving in a cat, which is an animal very vigorous.

'Tis known that, in a dying animal, the auricle contracts itſelf, and palpitates more frequently than the ventricle, and that it hath ſometimes three, four, and even ſix contractions during one ſyſtole of the heart. Perhaps, at this time, the auricle cannot propel blood enough into the heart, ſo as to excite a motion by way of a ſtimulus, after many contractions; and, perhaps, the blood, thickened in theſe laſt moments, paſſeth no more to the ventricle, which often happens in dogs which have a very viſcid blood. I have ſeen the right auricle ſo filled with this blood, that thereby it loſt intirely its irritability and power of contraction, which happens to the bladder when too full of urine; and the ſame phænomenon appeared, whether this coagulation was naturally formed, or whether I had occaſioned it by any acid injection. I have alſo ſeen the ſuperior appendix, with much labour, endeavouring to deſcend, without being able to diſcharge its blood; at which time the inferior part of the auricle was at reſt.

More-

Moreover, the contractile power of this auricle preſerves itſelf generally very long; becauſe, after the ceſſation of motion in the left ventricle, cold, weight, and other cauſes, continue to determine the blood towards this auricle, wherein much is generally found after death, and it is the common ſeat of Polypus's.

I have obſerved its laſt motion to be made in the inferior part, at the inſertion of the Abdominal Cava; at other times, the appendix is the laſt in motion. The right ventricle being filled with blood, extends itſelf, recovers, enlargeth itſelf, and, ſtimulated by the blood, even contracts itſelf, in the following manner: its motion deſcends from the baſe towards the diviſion of the ventricles, at the ſame time the parieties approach each other, and the cavity diminiſheth. On opening the ventricle at this time, I have clearly obſerved, that the tendons of the valves were relaxed; the ſame appeared on opening the left ventricle. At this time the heart reſiſts the finger, when it is brought into contract with it outwardly, and binds it when it is introduced into its cavity. In a cat, theſe motions are performed with but little force.

When the animal begins to languiſh, the right ventricle ſeldom contracts itſelf, and in an imperfect manner; and its motions degenerate into particular tremors of ſmall portions of fleſh, which have diſtinct motions of palpitation in the middle of other portions of fleſh which are at reſt; the laſt motion is made at the Apex.

From the right ventricle, all the blood naturally paſſeth into the pulmonary artery, with more force than is generally ſuppoſed; for I have ſeen, in a dog, the blood of this artery to ſpring to the ſame height as that of the Aorta. In a dying animal, the valves of the veins do not exactly meet and cloſe; and a great part of the blood of the right ventricle re-paſſeth into its auricle. I have obſerved alſo in a living cat, that the blood returned into the auricle by preſſing the heart. In frogs, we obſerve very diſtinctly for ſome hours, the blood and air to paſs from the auricle into the ventricle, and from the ventricle into the auricle, which is a proof that the valves do not exactly cloſe: it is alſo as certain, that in a living animal, all that part of the blood of the ventricle contained between the venal valve and the auricle, doth not re-enter the auricle at every contraction of the heart, as Rouhault hath juſtly obſerved (*c*).

The motion of this ventricle ſubſiſts much longer than that of the left, but not ſo long as the right ear, which is almoſt always the laſt part in which the blood collects.

(*c*) Oſſervationi fiſiche Anatomiche, p. 80.

We ſhall not trace the blood of the pulmonary artery in its courſe through the lungs; it is ſufficient to ſay, that, at its return, it is received into the left ſinus, and into the ſmall appendix of this ſinus, called the auricle. This auricle, in contracting itſelf, deſcends towards its baſe, and becomes ſhorter in emptying itſelf: I only once obſerved the auricle and the left ſinus only to ſtraiten themſelver without becoming ſhorter; ſo that the poſterior pariety touched the anterior, and the point of the auricle did not deſcend.

This auricle hath ſomething particular, which is, that it trembles and palpitates with a much greater ſwiftneſs than the right auricle, although the intire contractions are not more frequent than thoſe of the latter.

The motion of this auricle ceaſeth, and it loſeth its irritability before the ventricles, if the blood is not intirely coagulated in the parts of the heart on the right ſide, or if the Aorta is not tied up: for, having coagulated the blood by an injection with vinegar, not only the left auricle continued its motions, even after the right, but moreover it became at that time the only part in which we could recal the ſuppreſſed motions. I read before you, gentlemen, ſince the year 1751, expements with reſpect to a ligature on the Aorta, and its effects on the left auricle (*d*). It is not to be doubted, that in a living animal in health, the left auricle diſchargeth itſelf into the ventricle of the ſame ſide, but doth any part of its blood paſs again into the pulmonary vein? This I will not decide: analogy ſeems to perſuade it, from what is obſerved with reſpect to the right auricle and Vena Cava in animals that are near death; but the ſituation of the ſinus makes it very difficult to obſerve it exactly.

The left ventricle deſerves more particularly to be called the heart, ſince the ſeptum and the greateſt part of the Apex of the heart properly belongs to it, and makes only one body with it; alſo it moves different from the right ventricle: for, its fleſhy fibres re-aſcend all intire from the point, the origine of motion being there, from whence it proceeds towards the ſuperior part and towards the ſeptum; and the whole heart, as I have obſerved in a cat, aſcends towards its baſe and towards the Aorta: at which time the fleſhy parieties approach each other, and ſtraiten the cavity of this ventricle. During this motion, all the fleſhy fibres of the heart become wrinkled by tranſverſe folds; the ſame is obſerved in the right ventricle. At this time the pulmonary artery and Aorta are

---

(*d*) Comment. tom. i. p. 273, and the following; it is the ſmall diſſertation re-printed after this.

drawn downwards. The ſtrength of this ventricle a little exceeds that of the right.

The principle, and almoſt ſole cauſe, why the point of the heart is thruſt forward, is the ſituation of the left ſinus. When we inflate this ſinus, after having opened the breaſt, the point of the heart is ſeen briſkly to draw near the nipple. In frogs, the auricle is placed behind the heart; and it is found in its diaſtole, when the heart is in its ſyſtole. Its motion alſo contributes to move the point of the heart forwards: but whether the Aorta be full or empty, it doth not appear that this produceth any change in the ſituation of the heart in this animal.

The point of the heart preſerves its irritability very long, and keeps in motion longer than all the other parts of this ventricle. I have obſerved the ſeptum to tremble and palpitate, after all the parts of the heart, and even the auricles, were intirely at reſt. Moreover, I have obſerved ſometimes in cats, the ceſſation of motion to begin at the left auricle, and afterwards it took place in the ventricle on the ſame ſide, and alſo after this in the right ventricle, at a time when the right ear appeared the laſt in motion.

This is the place to examine whether the point of the heart recedes from, or approacheth its baſe in the ſyſtole. I will not quote all the authors that have treated on this matter. It is ſufficient for me to be able to aſſert, that in a very great number of experiments made on dogs, cats, kids, ſheep, rabbits, mice, hedge-hogs (*e*), hogs, and frogs, I have conſtantly ſeen, that at the time of the contraction of the left ventricle, the point of the heart raiſeth itſelf, and, approaching the baſe, comes into contact with the breaſt; but, in the diaſtole, as the ventricle fills, the heart lengthens itſelf, and extends itſelf in a very ſenſible manner. I have ſeen in a frog, which is a very little animal, this point to draw near the ſternum a whole line in the ſyſtole, and the heart conſequently to bend in proportion: and, in the ſame animal, I have obſerved this contraction and ſhortening, at a time when the ſyſtole ſenſibly appeared, although the arteries were tied up, and, conſequently, no blood could go out from the heart.

The eel, whoſe heart is particularly formed, is larger below, and terminates in a point near the Aorta, is the only animal, whoſe heart can be ſaid to lengthen itſelf at the time of its contraction; for it extends its point from the Aorta below as far as the liver: at the time it conveys its blood

---

(*e*) Permit me here to ſay, as I have an occaſion to ſpeak of this animal, that in it I always found a Pericardium and its blood warm, although the contrary hath been aſſerted.

into the Aorta. M. Queye (*f*) hath obſerved this in the tortoiſe; and I cannot ſay whether the ſtructure of its heart is the ſame with that of the eel.

This motion of conſtriction compreſſeth the blood of the left ventricle, into which, on introducing the finger, a compreſſion may be perceived, as I have before related with reſpect to the right ventricle.

If the point of the heart is cut at this time, the blood is driven from the ventricle. This blood is naturally propelled into the Aorta, which becomes ſwelled, and, eſpecially, if the artery was before tied up: for the heart, irritated by this ligature, labours ſtrongly to free itſelf from its contained blood; and the number and force of the pulſations increaſe even in the eel, which is a cold and ſluggiſh animal (*g*).

When the heart is abſolutely empty, it remains intirely at reſt, and, its folds diſappearing, its ſurface becomes ſmooth; and, in this ſtate, which is called relaxation in the muſcles, it becomes ſoft, extended, ſtrait, and at reſt. It remains in this indolent ſtate without recovering its ſyſtole, not only for minutes, but when the animal is in a languiſhing ſtate for whole half hours. This continuance of a ſtate of reſt is alone ſuffi-

---

(*f*) Diſſert. de Syncope.

(*g*) See the diſſertation of M. Remus, p. 22.

cient to prove, that the diaſtole is not an effect of muſcular action, but of relaxation and weakneſs. A muſcular contraction hath never continued whole half hours.

A ligature on the Aorta doth not prevent, in the heart of a frog, the ſame ſymptoms which it hath in a ſtate of reſt; the heart extends, unites, and lengthens itſelf: it recovers again its ſtate of diaſtole, although it remains full of blood.

Moreover, the heart doth not become red at the time of the diaſtole, neither doth it become pale at the time of the ſyſtole; and repeated experiments have convinced me, that all which Harvey hath wrote, with reſpect to its changing its colour, only takes place in animals that have cold blood, in which we are able to trace by the eye, a column of blood, which appears ſucceſſively in the Vena Cava, in the auricle, in the heart, and in the Aorta; all which parts become red the time this red column is paſſing them, and become white when it paſſed off; but neither the fleſh of the heart, nor of the muſcles, ſuffer any change in their colour, whether the heart is in a ſtate of reſt, or in a ſtate of contraction.

The blood propelled into the Aorta is from thence diſtributed into all parts of the animal; but if the Aorta is tied up, I have ſeen it return into the heart, which proves, that in a living animal, the effect of the valves is not of ſo great a conſequence as is ſuppoſed.

Therefore the Vena Cava, the two auricles, and afterwards the two ventricles, are ſucceſſively contracted in the order above-related, and become relaxed in the ſame order; ſo that the dilatation, or the contraction of the Vena Cava, and of the two ventricles, always happen at the ſame inſtant of time. 'Tis an agreeable ſight to obſerve the gradation which enſues on the filling of theſe parts, in an animal that hath cold blood: at firſt, part of the auricle on the right ſide, which is behind the heart, ſwells with blood; afterwards the part on the left ſide of this auricle; afterwards the ventricle; and, laſtly, the artery, which goes off from the heart, paſſing obliquely before the auricle: this I have obſerved for nine hours.

Experiment falſified all that hath been wrote with reſpect to the alternate contraction of the two auricles or ventricles. The authors of this hypotheſis have made the right ventricle to contract itſelf before the left; and the celebrated Dr. Nicols (*h*), and Lanciſi (*i*), have been both deceived: the former hath made this difference of one pulſation; and the other only of part of a pulſation.

(*h*) Compend. Oeconom. p. 27.

(*i*) De Corde & Aneuriſmat. prcpoſ. 59, 60, 61.

But, on opening both the ventricles, the blood makes intirely, at the ſame time, two currents from the two ventricles. It is ſufficiently known, that the heart is very irritable; but nothing better diſcovers this truth, or revives its motions with ſuch certainty as the air; and its motion continue much longer, when they are produced by inflating the heart, than when they depend on the blood which it contains: alſo, in a dying animal, it recovereth again, by its own power, its motions, after it had been in a perfect ſtate of reſt; and, after ſome contractions of the right auricle, it ſeems to recover in ſuch a manner, that the intervals between theſe different revived motions, become always longer. The whole heart, in a living animal, hath one common motion; and when the ribs aſcend in inſpiration, the heart ſinks with the Diaphragm, and aſcends again at the time of expiration. Alſo the Venæ Cavæ are drawn by the Diaphragm, and deſcend at the time of inſpiration. Thoſe who believe that part of the Diaphragm which ſupports the heart immoveable, ſeem never to have opened living animals. The contractile force of the arteries is generally conſidered as the ſecond cauſe of the motion of the blood. Many authors ſuppoſe this power equal to that of the heart; and others believe it more conſiderable (*k*). Many believe that the force of the heart is ſufficient to propel the blood, even into the ſmall arteries; and that the force of the arteries is the cauſe of its return into the veins (*l*). On this reaſoning, a want of pulſe in the veins is commonly accounted for. It is believed that the venal blood, being alternatively propelled by the force of the heart, and by that of the arteries, preſerves an uniform motion without intermiſſion (*m*). We find in animals that have warm blood, and in man, that the arteries, even the ſmalleſt branches of the brain, have red fibres, ſuſceptible of contraction, and capable of cauſing a conſtriction; and this force is proved by experiments. When an artery is tied up, the part under the ligature propels equally into the veins it contained blood (*n*). If two ligatures are made on the artery, the blood contained between the two paſſeth equally into the neighuouring branches (*o*). When the Aorta is oſſified, the Vena Cava becomes filled with blood coagulated and im-

---

(*k*) M. Senac. This celebrated phyſician believed, that not only the force of the arteries preſerve the force of the heart which dilates them, but alſo that it multiplieth it. Traite du Cœur, tom. ii. p. 166. See p. 199, 200. 224, 225, &c.

(*l*) Pechlin de Corde, N° 21. Thomſon Diſſertat. 1. Moriſſon on the Choice of Bleeding.

(*m*) M. Sauvage Pulſus Theor. p. 26.

(*n*) Drelincourt Canicid 1. Pecquet. p. 46.

(*o*) Schwenke Hæmatolog. p. 80.

moveable (*p*). We have other experiments of this kind.

Although I have no thoughts of refuting theſe facts, I think it becomes me to report what anatomy and experiments have taught me with reſpect to this matter. In the firſt place, the arteries of frogs have always appeared to me deſtitute of any power of contraction, when I conſidered the perfect equality of their diameter in the ſtate of inanition and repletion, or when I examined the effects which poiſons made on them, the moſt corroding of which have never been able to produce in them the leaſt contraction (*q*); and, on obſerving their compoſition, which is much like the cellular net-work, and which hath not any one fleſhy fibre; and, laſtly, on reflecting on their want of pulſe, ſince it appears that the veſſels which have no dilatation, have conſequently no contraction; and that the arteries and veins of frogs, touched with ſpirit of nitre, have ſuffered no change, neither do they contract tnemſelves: although this poiſon produceth even a change of the blood in the veſſels, and gives it an earthy colour, and of the conſiſtence of dirt, nothing in a manner can be concluded from the inanition of the veſſels. In frogs, the arte-

---

(*p*) Sanctorini de Nutritione.

(*q*) See in the Diſſertation of M. Remus, p. 48. The experiments which we made together.

ries are often intirely empty; and after bleedings, under the ligatures, and in other circumſtances, we obſerve the globules of blood gradually to forſake the artery, until it becomes intirely empty and white. Theſe veſſels empty themſelves ſo intirely, that not one ſingle globule remains; but I have been convinced, by many experiments, that the globules are moved in the arteries in theſe caſes, even without any contraction of the veſſels, and independent of the force of the heart. In an artery almoſt empty, one ſingle row of globules is obſerved, at the extremity of the artery, to advance, to ſtop, to move in a contrary direction, and at laſt to diſappear; ſo that the beſt microſcopes are not able to diſcover the ſmalleſt motion in the veſſels, or any diminution in the diameter of the artery, after the blood hath left it.

With reſpect to the arteries of animals that have warm blood, and which have an apparent pulſe, I agree that they have ſtrength ſufficient to recover themſelves after they have been dilated; this ſtrength depends on the circular muſcular fibres.

But we ſhall be eaſily convinced, even in this ſpecies of animals, that the force of the heart is very ſuperior to that of the arteries, if we conſider what follows. Firſt, with reſpect to the early force of the heart in the firſt times of a fœtus. This organ propels the blood through gelatinous arteries. We may live very long, although almoſt all the arteries are oſſified; ſince we find often in dead ſubjects, a continuation of bony Laminæ from the head to the foot, between the muſcular and internal coat of the arteries of people, who, when living, followed their buſineſs, and complained of no indiſpoſition depending on the irregularity of the circulation (*r*). For, although in the end a ſphacelus terminates this ſtate, when the arteries are no longer ſuſceptible of any dilatation, yet they may live many years before a ſphacelus enſues; and theſe old men, whoſe arteries have thus become oſſified, uſed the exerciſe of walking at this time, have had a pulſe, preſerved their natural warmth, and followed their buſineſs a conſiderable time. Nobody will ſuppoſe, that a degeneration like this, can be the work of a ſmall time; and it is manifeſt that much time is required to give a glutinous liquor the hardneſs of a bone. We muſt yet compare the great irritability of the heart, which inflation, or every other

---

(*r*) This is frequent enough in old men. Harvey relates two examples, de Circulat. Sanguinis, p. 218, without mentioning any inconvenience in the life-times of theſe men. The caſe which I have obſerved, may be ſeen in the Philoſophical Tranſactions, N° 483. Opuſcul. Patholog. Obſervat. 46. 52. and thoſe which other authors have obſerved in the Philoſophical Tranſactions, N° 299.

mechanical irritation, procures, with the perfect Inertia and weakneſs of the arteries, which may be irritated in a living animal by the knife, the needle, poiſon, or by any other means, without their contracting themſelves the leaſt in the world (*s*). Laſtly, it muſt be conſidered, that the motion of the blood recovers itſelf again in a man that hath been drowned; and this only by irritating the heart. If the heart alone is able to make the blood circulate, in immoveable veſſels, in animals that have cold blood; in animals that have warm blood, which have proportionably a heart much larger than the others (*t*), it will be able more eaſily to produce a circulation without any foreign help.

I ſpeak not here of the mechanical contraction of arteries that are dried up, or of the effect reſulting therefrom: as the membranes become dry, they either expreſs and diſcharge their blood, or they change it into

---

(*s*) Comment. Societ. Reg. Gott. tom. ii. p. 131. 141. Diſſertation on the irritable and ſenſible Parts, ſect. 11.

(*t*) Robinſon, of Food and Diſcharges, ſaith, that in uſing the mean terms, the heart of a cow, of a bird, and of a fiſh, is in the following proportion, $\frac{1}{263}$, $\frac{1}{168}$, $\frac{1}{136}$. That is to ſay, that proportions conſidered relative to the maſs of the animal, the heart of a very ſlow quadruped is more than five times larger than that of fiſh. Confront. p. 107. with p. 120.

fibres and membranes, and the artery becomes filled with a cellular marrow; but theſe changes take their riſe from dead force, and continue their action for years after the circular fibres have loſt theirs: an example whereof we have in the umbilical arteries, in the arterial canal, and in Aneuriſms.

Of this nature, I ſuppoſe that force which makes an artery that is cut, to ſhrink *(u)*; and which is yet ſtronger and more ſenſible in the tendons or ligaments, although they are neither hollow nor irritable.

Alſo the motion of the muſcles is reckoned one of the powers that help the circulation, which acts chiefly on the veins; but I ſpeak not here of that motion, by which it is ſaid, even by the moſt modern authors, that the blood is driven from the muſcle during its contraction. For, having often examined with a microſcope, the inteſtines and the great muſcle on the leg of a frog, at the time of their contraction, I have obſerved that, in in theſe parts, the blood is ſeen in them, and moves in their veſſels, both before the contraction and in the following relaxation, and that the arteries remain equally full, both at the one time and the other; but the time of the contraction is ſo ſhort, and the focus of the microſcope ſo ſuddenly changeth, that I know

---

(*u*) M. de Sauvage Theor. Tumor. p. 8.

know not any method to obſerve a muſcular fibre at the inſtant of its contraction. Alſo, the argument that is made uſe of to ground this hypotheſis is (*x*) taken from a ſtrong current of blood, on moving the arm at the time of bleeding; but this does not prove what they would infer from it. For, whoever hath obſerved the rapidity with which they force the blood out of the vein, by compreſſing and turning in their hand any cylindrical inſtrument, cannot be perſuaded that this blood was expreſſed at this moment of time, from the capillary arteries of theſe muſcles into the veins and venal trunks, and, laſtly, into the vein that is opened by the ſurgeon. Moreover, an experiment very eaſy, and which may be repeated on any animal, proves that the muſcles do not become pale when they act, which muſt happen if their blood left them at this time. All this ſyſtem is founded (*y*) on Harvey before cited; and, through an inconſiderate analogy, they have attributed to all the muſcles a property which is peculiar to the heart, as it is a reſervoir of blood, but not as it is a muſcle.

But the muſcles contribute, in a different manner, to the circulation of the blood: they compreſs the veins which lie between them;

(*x*) Memoire ſur le Mouvement des Muſcles, Nº. 20. p. 82. Recueil pour le Prix de l'Academie.

(*y*) Pages 22 and 23.

and this compreſſion, whoſe effect is directed by the valves, contributes to haſten the blood from the ſeat of the heart. This is the true cauſe why the force of the blood, at the time of bleeding, is increaſed, by moving in the hand ſome inſtrument, or by muſcular efforts of the wriſt; it is alſo the cauſe of a phænomenon often mentioned by Boerhaave: that is to ſay, when the lower belly of an animal is opened, the veſſels of the Meſentery and of the inteſtines fill with blood, ſo as to become almoſt varicous, by having loſt this auxiliary contraction which the muſcles of the lower belly produced in them. It is this effect of muſcular motion, which is the cauſe that nobody can deprive himſelf of it for any conſiderable time, without ſuffering a diminution of the circulation, eſpecially in the feet, and which would produce in them a continual coldneſs and an œdema: from hence I account for the ſtones, which are ſo frequently found in the gall-bladder of criminals which have been a long time in gaol, and many other the like phænomena.

I have with care examined a particular muſcle, to ſatisfy myſelf in what manner it aſſiſts the circulation of the blood; I mean the Diaphragm. The late M. Walther having oppoſed what I had advanced concerning the force this muſcle uſeth in binding the Vena Cava, I made a great number of experiments on dogs, cats, and other like animals; and I obſerved, after having opened the lower belly, that, at the time of inſpiration, the ribs aſcend, and the Diaphragm deſcends; and that, by this motion, the Vena Cava is drawn down and conſtringed, ſo as to empty itſelf and become pale: and it is evident that the ſame thing muſt more reaſonably take place, if the lower belly is not opened; and that its cavity being intire full, all the parts are brought more into contact. The moment following, when the Diaphragm becomes relaxed, the Vena Cava re-aſcends, and becomes filled with blood, which returns from the Abdomen. This motion is ſo apparent, that I ſhould have attributed to it the phænomena which I have obſerved in the brain, and which I have ſet forth elſewhere, if I had not obſerved the ſame changes to have happened in the arm and neck, which are parts to which this action of the Diaphragm cannot extend. This remark diſcovers to us a new effect of reſiſting powers, which is that, by reaſon of the length of inſpiration, they hinder the return of the blood from the inferior parts of the lower belly.

I have formerly treated on the effect the nerves have on the arteries. M. Mekel hath lately treated on the ſame (z). M. Senac believes this effect very conſiderable; and that the nerves have a power of extending and relaxing the arteries (*a*), to ſtop the motion of the blood in the very ſmall ones (*b*), and to produce other the like phænomena.

(z) Memoires de l'Acad. Roy de Berlin, 1751. The title is 1752.

It is certain, that conſiderable nerves are found near the carotid and its large branches, but we cannot prove that they make a part of, or are diſtributed into, the ſubſtance of the arteries, ſince the arteries in general are almoſt inſenſible, and may be tied up in man and animals without pain. On the other hand, the ſudden changes produced in the circulation by the mind, the erection of the Penis, of the Clitoris, of the nipples of the breaſt; the changes of colour in the cheeks; Cold and heat, Tremors, Inflammations, Obſtructions, the ſtrong motions which violent pains or wounds of the nerves often occaſion. All theſe facts conſidered, render very probable the action of the nerves on the arteries; ſince it by this appears, that they have certainly a great power, either of retarding or accelerating the circulation.

But I have never been able by any experiment, to find by obſervation, that irritating the nerves created any change in the velocity of the humours contained in the veſſels of the

---

(*a*) Traitè du Cœur, tom. ii. p. 108. 209.
(*b*) Ibid. p. 170.

circulation. I have indeed obſerved, once or twice, that, by irritating the nerves, it revived the action of the blood, and reſtored the motion of it in arteries that were cut, which had ceaſed to ſupply any. But I have had great reaſon to attribute this effect to ſome mechanical action (*c*). By irritating the phrenic nerve in large animals, I have never found that the pulſations of the heart became altered; and another obſervation, which is very material, is, that in hyſteric paroxyſms, in the Tetanos and Emproſtotonos, often the pulſe is neither increaſed or altered, notwithſtanding the violent agitations the nerves and muſcles undergo. Moreover, by irritating the ſpinal marrow, all the muſcles become convulſed, the heart excepted, which preſerves its regular motions; if the ſpinal marrow is deſtroyed, the heart preſerves its motion, and it continues to move in a frog after the head is cut off (*d*); but enough of this here.

Heat and cold are almoſt the only agents on the nutritious juice of vegetables; heat makes it aſcend, and cold the contrary. It is eaſily obſerved in men, that the cold air makes their hands pale, dry, and rough, that it forceth back all the blood, not only from the ſmall cutaneous veſſels, but alſo from the large

---

(*c*) Malpighi relates that a convulſion had reſtored the courſe of the blood. Oper. Poſthum. p. 92.

(*d*) Redi hath obſerved it in a tortoiſe.

veins on the back of the hand; and at length when all the blood is withdrawn, the whole part periſheth, and remains only a white inſenſible maſs (*e*). It is to the cold air, which binds and ſhortens every thing in nature, that I attribute the collections of blood that are ſometimes obſerved in the Vena Cava and right ventricle: for, ſince it is proved by the experiments of the celebrated M. Clifton Wintringham, that, in the animal body, the branches are every-where more denſe and firm, proportionally as their trunks; the ſame cauſe acting on all the veſſels, the effect of theſe conſtrictions will be greater in the ſmall veſſels, which are the more firm, and leſs in the large trunks which are more lax, and the force of the ſmall veſſels prevailing, they will empty themſelves into the great trunks. I do not ſpeak here concerning this effect of cold on the veſſels of reſpiration nor on the ſkin, which it hardens, ſo as to make it change the direction of its hairs; my thoughts whereof I ſhall reſerve for my phyſiology.

Heat, on the contrary, relaxeth and reſolves every thing; it ſeemeth to dilate the veſſels, and make them more eaſily give way to the action of the blood, and increaſe their diameter, which may be obſerved in the cutaneous veins of the hands. A greater quantity of blood is drove into the heated parts,

---

(*e*) Ellis's Voyage to Hudſon's Bay, p. 176.

which make the leſs reſiſtance; for the member ſwells and becomes red, either on being ſuſpended in warm water, or heated by frictions. On warming the veſſels of a frog, in which there is any coagulated blood, the coagulum diſſipates, and the diſengaged globules go off from the parieties; an experiment which Lewenhoeck hath accurately made on a bat, and which I have verified (*f*); but I have not made any freſh experiments with reſpect to theſe cauſes of the variation of motions. I ſhall now more fully ſpeak as to the effects of weight. It is not ſurpriſing, that this force contributes to determine the motion of the blood, not only after death, but even during life; as the blood is heavier than water, whoſe gravity alone produceth ſo great a motion in mechanical tubes. Andrew Paſta (*g*), an eminent phyſician at Bergama, hath very exactly obſerved, and alſo very well deſcribed, the effects produced after death by the weight of the blood. I have examined the effects of this force on living frogs. My motive for this examination was partly becauſe eminent men, and whom I reſpect, have denied that this weight had any influence on the circulation in living animals (*h*). This experiment is very

---

(*f*) Experim. & Contemp. tom. ii. p. 108.

(*g*) De Motu Sanguinis poſt Mortem.

(*h*) A. F. Walther de Acceleratione & Retardatione Sanguinis.

eaſily made: we need only elevate and lift up the whole Meſentery, having taken care not to wound it, and all the veins will be found to empty themſelves, and to look like white threads; looſe it, and leave the ſame veſſels to their own weight, and the body of the frog becoming higher than the Meſentery, you will find all the veins to fill themſelves with blood, and to receive again their red colour: uſe a microſcope, and obſerve ſome current that is perpendicular to the board on which the animal is extended; turn afterwards the whole machine upſide down, that its inferior extremity become the ſuperior: obſerve again, and you will apparently find this torrent of blood inverted, and to move in a contrary direction to what you had obſerved, and to deſcend inſtead of aſcending. This is eaſily repeated; it becomes more apparent after the animal is dead, or when in a languiſhing ſtate: but the circulation in the veins is never ſtrong enough, ſo as not to undergo an alteration by the reſiſtance of the weight. This effect of gravitation appears but little in the arterial blood, unleſs its motion hath began to abate; in that caſe it becomes retrograde, and is propelled by its weight. By this we ſee that, even in a living animal in health, the venal blood very eaſily moves, as its weight directs it, and with a greater difficulty in the contrary direction; and that ſometimes, in this caſe, it falls again towards the parts it comes from. This teacheth us the true cauſe of the Incubus, and of that heavy drowſineſs of a perſon that ſleepeth lying on his back, or that ſleepeth in a perfect level; for his blood aſcends to the head more eaſily than it would in a ſituation not ſo ſtrait, and it returns from thence with more difficulty, becauſe it is not aſſiſted in its return by the force of its weight. Thus we ſee why the feet are ſubject to Varices, to œdematous tumours; and why they become cold the firſt, although covered. Theſe effects are produced, becauſe the blood returns with difficulty from the inferior parts of the body, from whence it muſt aſcend, contrary to the force of its gravity.

From hence alſo we may eaſily comprehend why the ſoothſayers among the Romans, obſerving the flight of birds had varicous legs. Why printers, and other tradeſmen, are ſubject to œdematous legs. Why the blood flies to the eyes of people that read ſmall characters, or who employ themſelves in work that is extremely fine. This defluxion of humours on the eyes extends them, making the Cornea protuberant; and theſe people become what we term Myopes. We ſee alſo why thoſe who ſtoop much in reading, are ſubject to a troubleſome rheume of the head, created by a ſtoppage of the blood in the veſſels of the noſe. We diſcover alſo by this ſame phænomenon, the neceſſity of the valves in the limbs, eſpecially in the feet, whereby the blood of the Abdomen is hindered from reſiſting, by its gravity and weight, the return of the blood from the feet, either in ſitting, walking, or ſtanding. I have before taken notice, that I have not been able to diſcover any valve in frogs, nor in the human Meſentery, which is the cauſe of the hæmorhoides: I do not believe any author hitherto hath proved by facts, the power that gravity and weight hath of retarding in a living animal the motion of the blood, or of giving it a retrograde motion.

The air dilating itſelf in the blood after death, occaſions various motions (*i*). I have often, after having broke the bronchial veſſels, obſerved it to diſperſe itſelf through the Trachea and mouth, eſpecially in women dying in childbed, or in others dying of ſome ſudden and malignant fever: this probably is the only cauſe of the Vampyrs. It is certain, that I have ſeen the blood that was found in the heart of a young man filled with air, to reſolve itſelf into froth, and to diſcharge itſelf from the opening I had made in the heart. At another time I ſaw the blood to diſcharge itſelf from the mouth of a very fine woman, who died in

---

(*i*) Lanciſci de ſubitanea morte, I. C. p. 16.

childbed, ſo as to fill the ſhroud. Thus Schurig hath obſerved in an apoplectical perſon, the blood to come out of the mouth with much froth and noiſe (*k*). Hildanus hath alſo ſeen blood diſcharge itſelf from the mouth of one that had been drowned, ſixteen hours after his death (*l*). Caſes of this nature may be found every-where: and, perhaps, this is the only cauſe of the bleeding in dead people, which the ſuperſtition of the ancients hath regarded as an index of divine vengeance, which diſcovered the criminal by this bleeding his preſence had created in the body of the perſon he had deprived of life. It is to this expanſion of the air, produced by putrefaction, that deliveries which have happened to women after their deaths, are to be attributed.

The laſt article that remains for me to treat of, and which deſerves being more amply conſidered, is the motion of the blood, which continues after the heart is taken away, or the Aorta tied up, and which does not belong to any of the preceding cauſes. I have made for this purpoſe thirty-three experiments: in ſixteen I have cut off the heart, and, in ſeventeen, the two great branches of the Aorta; and in theſe laſt experiments I performed this operation without being able to obſerve any change in the veins. But,

(*k*) Sialographia, p. 405. (*l*) Cent. 3. Obſ. 12.

before me, Woodward had obſerved the motion of the blood to continue for ten minutes in the tail of a little fiſh that had been cut off (*m*).

Borelli was the firſt who taught us, that, after the heart was deſtroyed, the blood gradually removed out of the arteries, until they became abſolutely empty (*n*), after having taken away the heart, or, which is the ſame with reſpect to the arteries, after having cut off the two great branches which the Aorta of frogs ſends out at its riſe. In twenty-three experiments, the motion ceaſed ſeven times in the arteries of the Meſentery: in eight other caſes, the blood had a retrograde motion, and this retrograde motion was twice very rapid, and continued to a perfect inanition; in four other frogs, the blood balanced for near an hour, going and coming continually from the trunk to the branch, and from the branch to the trunk. Laſtly, in the four laſt animals, I obſerved the ſame direction of motion as in the natural ſtate; and this natural motion preſerved itſelf in a toad for twenty minutes.

We may infer from hence, that, after the heart is taken away, or the great arteries, the arterial blood hath yet continued its motions for a certain time, from ſixteen to twenty-one,

---

(*m*) Supplement. p. 102.

(*n*) De Motu Animalium, lib. 2. prop. 31.

twenty-ſeven, thirty, and thirty-ſix minutes, the more frequently it hath had an oſcillatory motion, leſs frequently a retrograde one, and only once it continued to move in its natural order.

Bleeding five times reſtored the motion of the blood as in a living animal; ſo that when there was but very few globules in the artery, if, at that time, this operation was performed even fifteen minutes after the heart was cut off, the artery filled itſelf again with blood, which came at firſt very readily, and even againſt the force of its weight, but afterwards more ſlowly.

I have examined the motion of the venal blood in twenty-two animals, after having cut off the heart: in thirteen, it preſerved itſelf in its natural direction, for twelve to ſeventeen minutes; in three others it took a retrograde motion, and returned from the Meſentery to the inteſtines, twice it began at firſt to balance itſelf. I have obſerved four times both theſe motions to take place all at once in different branches; and, in one of theſe caſes, to continue for thirty minutes. I have ſeen in a vein which had three branches, the ſmall number of globules which remained in one of theſe branches, to move to and from the inteſtines: in another branch, the blood aſcended ſufficiently ſwift towards the heart for fifteen minutes, and afterwards re-

returned in an oſcillatory motion; laſtly, in the third, it deſcended alſo as ſwiftly from the trunk to the inteſtine, and returned from thence alternately. I have obſerved in a toad, that the motion of the blood preſerved itſelf for fifteen minutes after the diminiſhing of the Aorta; in the capillary network of the veins of the Meſentery, the ſingle globules paſſed at firſt into veins of two globules diameter, afterwards into thoſe of three, and from theſe into the venal trunks.

It hath conſtantly happened in all theſe experiments (whereof I have made ſeventeen) that the blood which had not any motion, recovered itſelf by bleeding, as in the life-time of the animal, and moved ſwiftly by the veins of communication, in all directions to the orifice of the vein; and this I obſerved twenty minutes complete after the death of the animal.

I have ſeen an oſcillation, ſufficiently ſwift in the veins, to ariſe not only immediately after the death of the animal, but even many minutes after. This motion continued in the veins of communication for twenty-ſeven, and even for thirty-ſix minutes; and, in general, whether the heart remained, or was taken away, the branches which join two trunks by an Anaſtomoſis, have always appeared to be the laſt parts wherein the motion of the blood was preſerved, as I have already declared elſewhere.

In order to diſcover the cauſes of this life without the heart, I firſt examined if this motion was produced by gravity; I have found alſo in theſe experiments, that the oftener the blood moved, not only in the arteries, but alſo in the veins, againſt the force of its gravity and weight; that the inverſion of the board, on which the animal was extended, did not make any change in the direction of the blood; and that the velocity, with which it is carried towards the vein that is opened, much ſurpaſſeth the gravitating power.

It is alſo very eaſy to be ſatisfied, whether this motion depends on the contraction of the veſſels. I have proved that, in frogs, the veſſels have no contraction; I have often ſeen ſolitary globules moving in arteries almoſt empty; and, what is more deciſive, I have ſeen globules that were diſperſed between the Laminæ of the Meſentery, to move, oſcillate, aſcend, and deſcend, as conſtantly and as ſwiftly as thoſe which were included in the veſſels: I have ſeen them aſcend and deſcend along the external parieties of the artery and the ſides of the inteſtines, and, afterwards, after having made a kind of Parabola, to return by a narrower and a more rapid torrent, and diſtribute themſelves between the Laminæ of the Meſentery. This motion doth not abſolutely depend on gravity, and the blood-globules aſcend as ſwiftly as they deſcend; and it is evident that the motion which is performed out of the veſſels does not depend on their contraction.

Doth this motion depend on the ſuction of the ſmall veſſels? Eminent modern phyſicians, ſome of whom are my particular friends (*o*), have attributed to theſe ſmall veſſels the power of ſuction, by which they draw the blood from the great ones; and they ſuppoſe them capable of aſſiſting the circulation.

But the blood of an animal, whoſe heart is deſtroyed, doth not move only from great veſſels to ſmall ones, but it more frequently moves from the ſmall branches to the great ones. In twelve experiments on the arteries, I have obſerved this laſt motion in eight caſes, and the firſt only in four. The ſame thing takes place with reſpect to the veins: in eighteen experiments, I have obſerved the blood in thirteen animals, to move from the branches to the heart, to balance itſelf in two others, and in only three to turn its courſe from the ſeat of the inteſtines.

I imagine nobody will think it probable that the capillary veins ſhould be able to bring

---

(*o*) M. Kenger Phyſiol. German. p. 25. This ſentiment is delivered in a diſſertation, whoſe title is, De Suctione Vaſorum Capillarum in Corpore Humano, a Joſ. Brun.

back the blood from their trunks, and make it turn its courſe contrary to the natural order of circulation.

For, if nature had given them this power, ſhe would have made them inſtruments both for the deſtruction of the circulation, as of the animal.

What therefore is the cauſe of this motion of the blood after death? it is or is not eaſy to declare. Let us endeavour to find it. I ſee by experiments, that the blood is ſtopped by the ſides of the Meſentery, and of the membranes, and by the lips of wounds: cut an artery or a vein, 'tis equally the ſame, either with the Meſentery, or in the adjacent part of ſome conſiderable wound of this large membrane; not one drop of blood will come from this opening you have made in the Meſentery. It appears therefore, that the blood is ſtrongly attracted by the membranes of the human body. I find alſo that effuſed blood conſtantly ſtops at theſe cellular lines, which every-where accompany the great veſſels; and that not only it continues in theſe lines, but that manifeſtly it directs itſelf towards them: the ſame is true with reſpect to the membranes of the inteſtines.

I have conſtantly obſerved that the globules reciprocally attract each other, and that if there was any blood collected in any conſiderable arterial trunk; in that caſe, the blood of all the adjacent branches flowed thereto. The ſame is true with reſpect to the veins. Alſo when an Aneuriſme is filled with blood, that of the ſmaller branches moves towards this maſs; and if it paſſeth from thence, it moves on towards ſome other maſs of globules contained in ſome part of the artery. When the blood is collected in two different places, an oſcillation ariſes between theſe two kinds of magnetic maſſes, which attract the globules that are between them, and make them move in oppoſite directions. The venal branches empty themſelves into the trunk; and the blood is a long time balanced and ſwung by two oppoſite motions, until at laſt the force of one prevailing, draws it intirely to itſelf, or until the veſſels become dried up. I once obſerved, it is true, that the blood which diſcharged itſelf from a lacerated vein, and being effuſed between the Laminæ of the Meſentery, it manifeſtly had an oſcillatory motion, and was abſorbed by the vein; and, in a moment after, it was diſcharged from it again, until this vein and the adjacent branches were filled. The celebrated Dr. Hale hath deſcribed the like kind of oſcillation in a muſcle, whoſe humours were alternately repelled and attracted by the extremities of the veſſels (*p*).

From all theſe facts, we may, I think, probably conclude, that the blood collects itſelf

---

(*p*) Hæmaſtaticks, p. 96.

into a maſs in places where there is firſt any of this maſs found, and thus it is in all dying animals. For, as to the arteries and veins, all the blood paſſeth from the branches into the trunks, and the former empty themſelves, and the latter fill themſelves: therefore the direction of the blood into the veins is the proper and direct motion, and which is the moſt frequent; and the motion into the arteries is a retrograde motion, becauſe theſe two different motions tend both equally to bring back the humours into their reſpective trunks.

There muſt be another cauſe for this ſwift motion, which carrieth the blood ſo conſtantly, and with ſuch force, towards the openings of wounded veſſels, whether it be an inviſible and innate contraction of the parieties of the veſſels, or ſome other property, I know not. Laſtly, let what will be the cauſes of theſe different motions, it is worth obſerving, that forty-one minutes after having cut off the heart, when the blood was intirely in a ſtate of reſt, I ſaw frogs make uſe of their muſcles, ſo as to jump and make their eſcape. This proves that the action of the muſcles doth not depend on the communication of the arteries, nor on any aſſiſtance from the blood. The ſame muſcles in the ſame frog conſtantly tremble by irritating their nerves, and remain always immoveable when they are cut off. This proves that the motion of the muſcles continues very ſtrong independent of the arterial blood, but, without the action of the nerves, it is loſt. Theſe frogs can ſee, although their heart is cut off; they draw their eyelids over their eyes, they breathe, and draw the air in at their noſtrils, which they can dilate, and give yet other ſigns of life. The eminent Caldeſi hath obſerved the ſame in a tortoiſe, which is an animal analagous enough to frogs (*q*).

Laſtly, gentlemen, what is abſolutely neceſſary to be known is, that I made all theſe experiments, which are very numerous, in the current of theſe twenty years laſt, on different kinds of animals. I have ſacrificed above eighty that have warm blood, and above ſeventy that have cold. I examined fiſh with the common microſcope of Culpeper, and ſeldom with the ſolar one, becauſe I obſerved that, though it magnifies objects prodigiouſly, it renders them confuſed, and their ſides not well adjuſted. I expoſed the frogs on boards, after the manner of Lieberkun (*r*), and I examined them with a glaſs which did not magnify extremely, yet is preferable to many others for its clearneſs. I have improved the inſtru-

---

(*q*) It lived two days without the heart, p. 76. Many of the like examples are found in the Prælect. Boerhavian. tom. ii. p. 614, 615.

(*r*) He hath given a deſcription of them in the Memoirs of the Academy at Berlin of 1745.

ment, by adding a part to keep the glaſs as firm as I would have it without the aſſiſtance of the hand.

It is not you, gentlemen, whoſe prudence, diſcretion, and ability, I acknowledge; but thoſe who neither know me, nor the love I have for truth: theſe are the cenſors, I ſay, whom I deſire not to paſs judgment on my performance, or on the account I give of what I have obſerved in one ſingle experiment or example. They will find nothing here, neither have they found any thing in my preceding experiments on reſpiration, irritability, and ſenſiblity, which I have ſeen once only; I have obſerved all with exactneſs, and have verified all more than once. When I ſaid that the Dura Mater, the tendons, and the Perioſteum, had no ſenſibility, I knew I oppoſed received opinions, which I could not expect would be given up, unleſs nature ſhewed herſelf on my ſide. When I deſcribed the uſes of the internal intercoſtals, and proved the abſence of air in the breaſt. I knew I ſhould find an antagoniſt in the late M. Hamberger; and that all the medicomathematical ſect, which were joined with him with reſpect to their opinions, muſt take on his ſide.

I have only related facts, which, to my ſatisfaction, I have proved by repeated experiments.

periments. M. M. Holman, Mecrel, Trendelenbourg, Sidren, Roederer, Sproegel, Oeder, Hahm, Zinn, Ith, Duntz, Runge, De Brunn, have been witneſſes, eſpecially of the experiments relative to reſpiration. I made thoſe proving the abſence of air in the breaſt before the whole ſociety of ſciences (*s*). M. M. Zinn, Walſtorf, Dethlef, Remus, Sproegel, Zimmerman, the Baron de Brunn, and many phyſicians amongſt thoſe I have named, have ſeen thoſe which reſpect irritability and ſenſibility (*t*). M. Remus hath aſſiſted at thoſe which reſpect the motion of the blood; and any body may ſee them, if he adds to ſome patience, a reliſh for truth, a freedom from prejudice, and a little art. As for thoſe who attack me, after having made one or two experiments, (or perhaps none at all) and unſucceſsfully, there are too many criticks, of this ſtamp, will be convinced of their error, when they attentively examine nature; and they will, with ſorrow, find themſelves obliged to confeſs

---

(*s*) The fourth of November, 1752. See the Nouvelles Litteraires of Gottingen, of the ſame month.

(*t*) M. Huerman hath verified at Copenhagen the experiments on the tendons. See his Phyſiolog. tom. iii. p. 79. and M. Tozzetti, Farion, Zinn, Rungf, Pozzi, Andrichi, Muhlman, Emett, and other phyſicians, have done the ſame, which will appear in the ſecond volume of Memoirs on the Senſible and Irritable Parts.

feſs that they have oppoſed truth: for, if I want either ſufficient health or talents for eſtabliſhing the truth, I have ſo good an opinion of mankind, as to aſſure myſelf that nature, at ſome time or other, will find her avengers, who will re-eſtabliſh her rights on the ruin of prejudice and opinion.

SUPPLEMENT

TO THE

PRECEDING DISSERTATION.

On the Cauſe of the Motion of the Heart.

Read the tenth day of November, 1751 (*u*).

HOWEVER ſhort this diſſertation may be, it will not be without its uſe. Herein will be ſeen an experiment, which I have many times made, and which proves that the motion of the heart, and its continual alternate contractions and relaxations, depend on irritation. All the demonſtrations that have been hitherto delivered with reſpect to this phænomenon, are deſtroyed by human or comparative anatomy.

(*u*) The experiments, on which this diſſertation is founded, are contained iu the 17th ſection of the Second Diſſertation on the Irritable Parts, and make the experiments 515, and the following to 523.

It is perfectly known, that the right ventricle, eſpecially its auricle, are the laſt parts of the body preſerving motion: experiments taught this to Galen (1), to Harvey (2), and to Boerhaave (3).

I have a long time ſuſpected (4), that the continuance of this motion depended on the blood with which the Venæ Cavæ, contracted by cold and compreſſed by palpitations and the weight of the muſcles, are continually furniſhing this ventricle: but the lungs of an animal dying, that are immoveable and collapſed, receive no more blood from the pulmonary artery; and that blood, which its contraction is able to propel into the left auricle, is very little, with reſpect to that which returns from all the body to the right auricle, to produce any ſenſible effect. It may then be ſaid, if the right ventricle and its auricle moves longer than the left auricle, that it is becauſe the venal blood ſupplies them longer.

I reſolved to verify my conjecture by experiments; and for this purpoſe, it was neceſſary to hinder, if poſſible, the entrance of the blood into the right ventricle: if by this means its motions ceaſed, it was a proof that they depended effectively on the receiving of this blood.

---

(1) Admin. anatom. lib. 7. c. 15.

(2) Diſſ. 1. p. 39, 44, &c.

(3) Inſtitut. rei Med. No 159.

(4) Commentar. in Boerh. t. iv. p. 609. Pprim. Lin. phyſic, No 113.

I firſt tried this experiment by ligatures; becauſe I recollected I had read in Bartholine (5) and in Berger (6), that a ligature on the veins made the motion of the heart ceaſe, and that it recovered itſelf when the ligature was cut off; and Harvey mentions his having made the ſame experiment on a ſerpent (7): but I did not ſucceed by this method; becauſe, as long as the animal continues warm, the blood that is in the right auricle continues to move it, although none enters it by the Venæ Cavæ; ſo that after having tied them up in three kittens, the motion of the blood ſtill continued. The ſame happened to Blanquet, in the experiments related by M. Senac (8). This made me reſolve to divide both the cavæ. I had intirely cut them off, but was afraid if I had, that the ceſſation of the motion of the heart, would be attributed to its want of the continuance of neceſſary ſupport; after having divided them, I diſcharged them of all their blood, and tied them up: I afterwards emptied the auricle, whereupon the ſucceſs of the experiment hath always been certain. When I had taken away all the blood from the auricle, and hindered it from receiving any more, it loſt immediately the leaſt appearance of motion, as it is more difficult to empty the ventricle than the auricle, and becauſe it gives way to the impreſſions which the left ventricle communicates to it; ſometimes a ſmall motion may be obſerved therein, incomparably more feeble than that which it hath when it receives the blood from its auricle and Venæ Cavæ.

---

(5) Anat. p. 376.

(6) De Nat. Hum. p. 62, 63, 306. See alſo Sorgeloos de Œconom. cor. p. 66, 69.

(7) L. C. p. 99.

(8) Traitè du Cœur. t. 1. p. 449.

I had alſo an experiment yet more authentic to make. In the natural ſtate, the right ventricle moves longer than the left, becauſe I have ſaid it receives the venal blood longer. In order demonſtratively to prove that the blood is effectively the cauſe of the motion of the heart, I had occaſion only to prove, that if the right ventricle and its auricle were deprived of their blood, at a time when the left ventricle was ſupplied therewith, that the former would thereupon loſe their motion, when that of the latter would be preſerved.

To ſucceed herein, it ſeemed at firſt neceſſary to empty intirely the right ventricle, by opening the pulmonary artery and the Venæ Cavæ, and to prevent the evacuation of the left ventricle by tying up the Aorta, and afterwards carefully to examine if in this ſtate, the motions of the right ventricle would ceaſe, and if thoſe of the left ventricle and its auricle were continued.

After ſome attempts, which the difficulty of this agreeable undertaking, and the haſty death of the animals had rendered unſucceſsful, the experiment anſwered my deſires; the right auricle remained intirely immoveable, and its ventricle preſerved only a motion, which was a neceſſary conſequence of the connexion of its fibres with thoſe of the left ventricle, and whereby its exterior parieties were drawn towards the ſeptum, which divides the two ventricles. The left auricle continued its motion for ſome time, the ventricle longer; and I have ſometimes obſerved, that at the end of two hours it yet contracted itſelf. The experiment thus happily ſucceeding, the blood aſcended from the point of the left ventricle to the baſe, and afterwards deſcended again from the baſe to the point; at which time the right ventricle, if it had any motion, ſeemed to deſcend: at other times I have obſerved this in a kid, and it had not any motion at all. This experiment more eſpecially ſucceeded, when the left auricle emptied itſelf freely into the ventricle, and when the blood had no acceſs into the Aorta that was tied up. The point of the left ventricle was always the part which preſerved the longeſt its motion. Thus may be transferred from the right ventricle to the left, the property of being the laſt part of the body in motion, by preſerving a longer time in the latter, the irritation that is produced by the contact of the blood.

A new force is given to this experiment, by endeavouring to inflate the right ventricle by this irritation; it revives and recovers its motions. Moreover, I have always obſerved, that the internal ſurface of the heart, is much more irritable than the external; for, on irritating this by the ſtrongeſt poiſon, the motion communicated to the heart ſoon ended; but, on irritating the internal ſurface only by air, it occaſioned, eſpecially in frogs and even in cats motions that continued very long, although all the parts were become cold. I repeated this experiment nine times, in order to preſerve in the left ventricle its motion, when all the orher parts had loſt theirs; ſeven times on cats, and twice on kids. The reſiſtance and great agitation in dogs, renders them improper for this experiment.

F I N I S.